PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From November 30, 1899, *to June* 14, 1900.

VOL. LXVI.

LONDON:
HARRISON AND SONS, ST. MARTIN'S LANE,
Printers in Ordinary to Her Majesty.
MDCCCC.

LONDON
HARRISON AND SONS, PRINTERS IN ORDINARY TO HER MAJESTY,
ST. MARTIN'S LANE.

CONTENTS.

VOL. LXVI.

No. 424.

No. 425.

No. 428.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

November 30, 1899.

Anniversary Meeting.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A full Report of the Anniversary Meeting, with the President's Address and Report of Council, will be found in the 'Year-book' for 1900.

The Account of the Appropriation of the Government Grant and of the Trust Funds will also be found in the 'Year-book.'

December 7, 1899.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The President announced that he had appointed as Vice-Presidents for the ensuing year—

The Treasurer.
Professor Dewar.
Sir Andrew Noble.
Dr. Johnstone Stoney.

The following Papers were read:—

I. Vapour-density of Bromine at High Temperatures. By E. P. PERMAN, D.Sc., and G. A. S. ATKINSON, B.Sc. Communicated by Professor RAMSAY, F.R.S.

II. *Polytremacis* and the Ancestry of the Helioporidæ. By J. W. GREGORY, D.Sc. Communicated by Professor LANKESTER, F.R.S.

III. Gold-Aluminium Alloys. By C. T. HEYCOCK, F.R.S., and F. H. NEVILLE, F.R.S.

IV. On the Association of Attributes in Statistics, with examples from the Material of the Childhood Society, &c. By G. UDNY YULE. Communicated by Professor KARL PEARSON, F.R.S.

V. Data for the Problem of Evolution in Man. III.—On the Magnitude of certain Co-efficients of Correlation in Man, &c. By Professor KARL PEARSON, F.R.S.

"On the Propagation of Earthquake Motion to great Distances." By R. D. OLDHAM, Geological Survey of India. Communicated by Sir ROBERT S. BALL, F.R.S. Received June 16,—Read November 16, 1899.

(Abstract.)

When preparing a report on the great Indian earthquake of 12th June, 1897, the author noticed that the European records of this earthquake showed a phase of increased disturbance in what are commonly called the preliminary tremors, making, with the great undulations, three phases of motion. He suggested that these three phases represented the arrival of (1) the condensational, (2) the distortional waves travelling through the earth, and (3) surface undulations travelling round the earth. The present paper is an attempt to verify these suggestions by a comparison with other earthquakes.

For this purpose a selection has been made from the published records of those earthquakes which fulfil the conditions (1) that the place of origin shall be known within 1° of arc, (2) that the time of origin shall be known within a limit of error of one minute, (3) that there shall be a sufficient number of records, distant more than 20° of arc from the origin, to serve as a check on each other. Eleven distinct shocks, representing seven great earthquakes, are found to satisfy these conditions, and in every case the same three-phase character as was recognised in the earthquake of 12th June, 1897, is found. A *comparison* of time intervals and apparent rates of propagation shows

that the coincidence is not accidental, but represents the separation of three distinct types of wave motion having different rates of propagation.

On plotting the records it is found that the time curves of the first two phases form curved lines, indicating an increase of apparent velocity with distance from the origin, such that, applying Rudzki's investigation, the wave motion represented by these two phases must have travelled through the earth, along curved wave paths, convex towards the centre of the earth, and with a rate of propagation which increases with the distance from the surface. On continuing these curves, by extrapolation, to the origin they give rates of propagation in very fair concordance with the rates of propagation of condensational and distortional plane waves which may be expected to obtain in continuous rock at some distance from the surface of the earth.

The waves of the third phase show no such increase of rate of progagation with distance from the origin. The rate of propagation is uniform at all distances; from which it is concluded that the great undulations of the third phase are surface waves, travelling with a uniform rate of propagation round the surface of the earth. It is also found that the waves of this phase set up by great earthquakes travel faster than those set up by lesser ones, and from this it is concluded that the rate of propagation of these waves is in some way a function of their size, thus affording a confirmation of Lord Kelvin's suggestion that their propagation is in part gravitational.

The general conclusion is that in the complete record of a distant earthquake, three distinct types of wave motion can be recognised (1) condensational, and (2) distortional plane waves, travelling by brachi stochronic paths through the earth, and (3) elastic, or gravitational elastic, surface waves, travelling round the surface of the earth. The records are, however, often incomplete by the omission of the first or the first and second of these phases, and the widely divergent estimates of the apparent rate of propagation of the preliminary tremors are largely due to this.

"The Medusæ of Millepora." By SYDNEY J. HICKSON. F.R.S.

Received November 7,—Read November 23, 1899.

Since the discovery of male medusæ in specimens of Millepora collected by Professor Haddon in Torres Straits eight years ago (2), I have examined several large collections of specimens, both dried and preserved in spirit, from different parts of the world with the object of comparing the medusæ and the ampullæ they form in the varying forms which the genus exhibits. The examination of the dried coralla in museums has convinced me that the presence or absence of ampullæ

cannot be used as the diagnostic character of any one species, since these structures occasionally occur in nearly all the principal forms of the genus which have been described. It was not until I examined one of the many specimens of the genus collected by Mr. Stanley Gardiner in Funafuti, however, that I had the opportunity of seeing again a male medusa, all the other specimens I had received having proved to be barren. The male medusæ of Mr. Gardiner's specimen turned out to be identical in size and form with those given to me by Professor Haddon, and I came to the conclusion that no specific distinction could be drawn between the two forms based on characters of the medusa before it is set free (3).

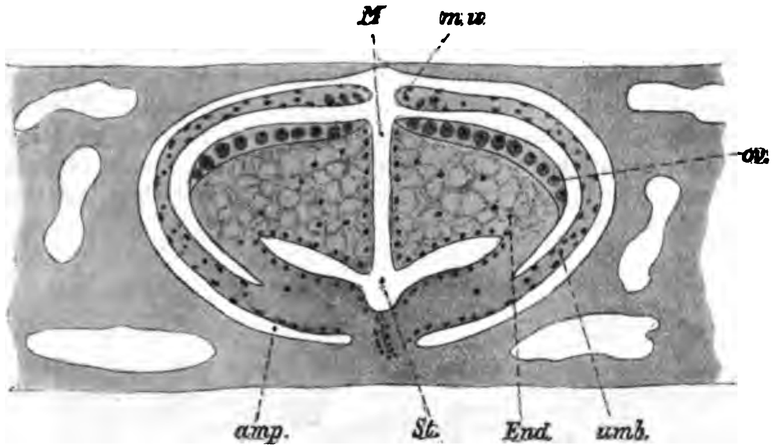
In May, 1898, Mr. Duerden, of the Jamaica Institute, sent me a small piece of Millepora preserved in spirit, which upon examination was found to bear female medusæ. Each of these medusæ carried on the manubrium from thirty to fifty ova of approximately equal size, 0·015 mm., and the general appearance of the medusæ suggested that they were nearly ready to escape. This appearance, however, proved to be deceptive, for in December I received another consignment of the material from Jamaica, in which the medusæ showed very different characters, three or four, or, in a few cases, five, of the ova in each medusa being greatly enlarged and the others degenerate. In the meantime, Mr. Duerden wrote to me saying that he had actually seen these medusæ escape, and had been able to liberate many others from the corallum by means of a needle. Thus was it definitely proved that this Millepore in the West Indies produces free swimming medusæ which bear ova.

Several important questions arose when this matter was settled. It was clearly important to find the male medusa of Millepore in Jamaica to compare it with the female medusa and also with the Pacific male medusæ. It was also important, if possible, to trace the development of the medusa, and to find, if possible, the place of origin of the sexual cells. In the hope of being able to get fresh material which would enable me to answer these and other important questions, I have delayed the publication of the results I have obtained for nearly twelve months, but as it seems probable that I may have to wait a very long time more before the material is forthcoming, I have decided now to publish the results of my investigation of Mr. Duerden's material.

1. *The immature female medusæ* received in May, 1898. A small branch of a Millepore well preserved in spirit after treatment with formalin was all that was sent to me. No very definite signs of the presence of the medusæ could be noticed before the specimen was decalcified, but as soon as the soft tissues peeled off the lower parts of the corallum under the action of nitric acid a considerable number of *medusiform bodies* could be seen with a lens. They varied consider-

ably in size, but the majority were about 0·4 mm. in diameter, and the remainder rather smaller. The structure of these bodies was examined by means of sections taken horizontally and vertically to the surface of the corallum. Each medusa (fig. 1) lies in an ampulla (amp.) or cavity in the corallum, and is attached by a narrow stalk to the centre of the innermost wall of the ampulla. The umbrella (fig. 1, umb.) is a thin

Fig. 1.

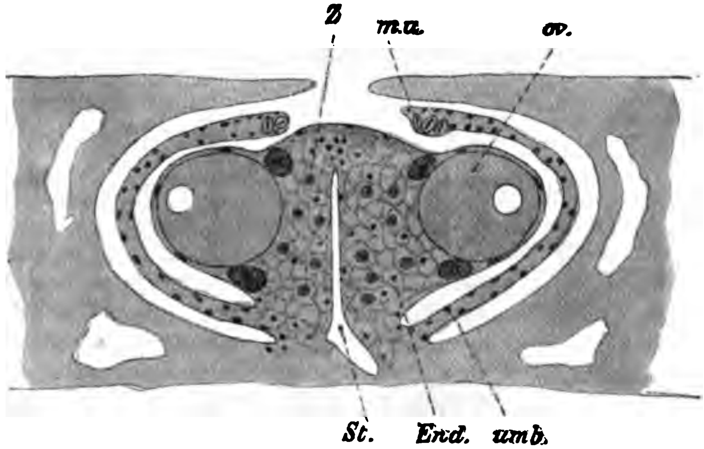


membrane, slightly swollen at the margin (m.u.). With a high power of the microscope it can be clearly seen that this membrane consists of a median lamella of endoderm, covered on each side by an ectodermal epithelium. No canals or cavities of any kind occur in the umbrella. There is no velum, and there are no tentacles. The cavity of the umbrella is almost completely filled with a swollen manubrium, which bears on its outer or upper side a mouth (M), around which lies a broad band of ova. There can be no doubt that in some cases there is communication between the endodermal cavity of the medusa and the sub-umbrella cavity by way of this mouth, but it is impossible to say in the present state of the inquiry whether the mouth of the medusa opens normally at this stage. It appears to be closed usually at the time when the medusa is ready to escape, as will be mentioned later on. The great size, however, of the manubrium of the female medusa is principally due to the thick vacuolated endoderm (end.). The gastral cavity of the medusa is not so simple as it is in the male medusa (of the Pacific Millepores), but it is subject to many very striking variations. In some of the medusæ there are four radial cœca proceeding a short distance into the endoderm of the manubrium from the main tubular gastral cavity that occupies the axis of the manubrium. This cavity communicates, on the one hand, with the exterior by the mouth,

and, on the other, with the canals of the cœnenchym. In some cases there are only three of these cœca, in others they are very irregular, and again in others there are none. It is quite impossible to determine with certainty what is the "typical" or "average" arrangement of the cavities of these medusæ with the limited amount of material at my disposal, but I think it is probable that the quadriradiate form will be found to be the most frequent of the many variations.

2. *The mature female medusæ* received in December, 1898. The material sent to me being abundant, I have been able to examine a large series of medusæ at this stage before their escape from the ampullæ. I have also examined a few medusæ which Mr. Duerden collected in his aquaria after their escape from the ampullæ. They are decidedly larger than the immature medusæ of the last collection; the diameter of the umbrella being about 0·6 mm. instead of about 0·4 mm. The variability is even more pronounced at this stage than

FIG. 2.



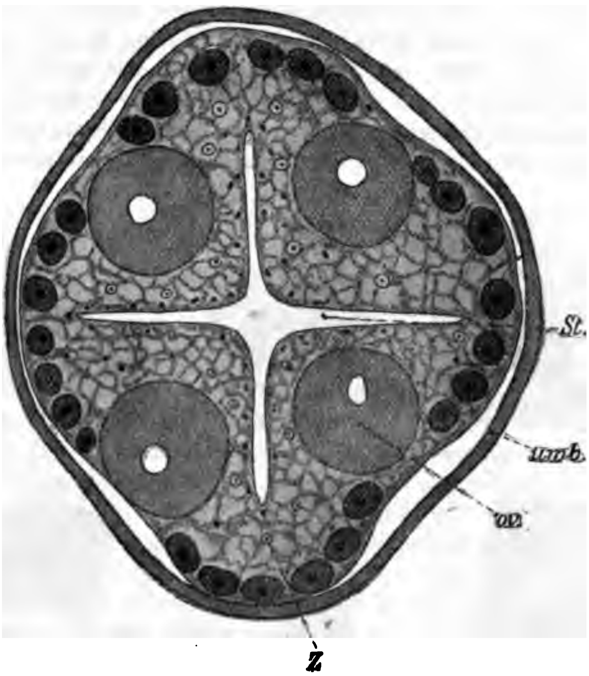
in the last, and it is almost impossible to find two medusæ exactly alike. In most of the medusæ (figs. 2 and 3) three or four large ova are found, and the manubrium is usually triradiate or quadriradiate accordingly, but occasionally medusæ with five, two, or only one mature ova occur, and the manubria of these are more irregular.

I mounted a piece of a decalcified specimen about 48 sq. mm. in area, and found that it bore forty-one medusæ; of these, nineteen had four ova, eighteen had three ova, two had two ova, one five, and one one. In another piece I counted three with four ova, six with three ova, and one with two. In another piece 15 mm. by 10 mm. in area I counted forty with three ova, fifteen with four ova, five with two, and five with

one. In this piece, however, I noticed that in one-third of the area the fours were about six times as numerous as the threes, while in the remaining two-thirds the threes were about nine times as numerous as the fours.

No very definite conclusions can be formed upon these figures, but it seems to me probable that so little is the form of the medusa of Millepora stereotyped, that a slight variation in the distribution of nourishment in the canals may be the determining cause of the medusa being triradiate or quadriradiate.

FIG. 3.



The germinal vesicle of each large ovum is regularly spherical in shape, with a very sharp outline. The nucleoplasm is apparently homogeneous, and resists the action of iron-hæmatoxylin and carmine, exhibiting no nucleolus nor chromatin granules.

In addition to the large ova, there are always present in the medusa several oocytes. These cells stain much more deeply than the ova, and many of them exhibit a well-defined nucleus with a deeply staining nucleolus. In some cases the cytoplasm of one of these cells may be seen to be continuous with the cytoplasm of an ovum, and I have little doubt that most of them, and perhaps all of them, are ultimately

absorbed into the substance of the large ova. The nucleus of the oocyte seems to be disintegrated before complete fusion takes place, since no degenerate nuclei can be found in the cytoplasm of the large ova.

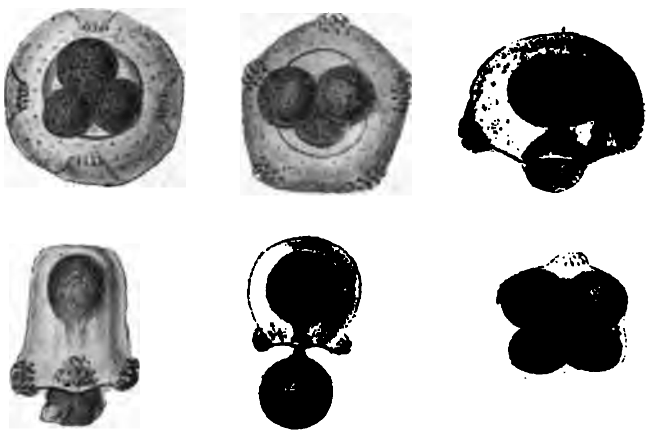
The remarkably thick endoderm of the female medusa is a very characteristic feature. It may be regarded as a special adaptation of endodermal tissue for the purpose of affording nourishment to the rapidly maturing ova, and similar in function to the trophodisc of the Stylasteridæ.

At this stage several zooxanthellæ (fig. 3, z) occur in the manubrium, but none were observed in the cytoplasm of the ova.

I have not been able to find evidence of the existence of an open mouth in the medusæ at this stage, but, bearing in mind the great variability they exhibit, I cannot assert that a mouth never occurs.

The margin of the umbrella exhibits three or four or five thickenings, due to clusters of nematocysts (figs. 4—9), but no definite tentacles nor sense organs were to be found after a most searching examination.

Figs. 4—9.

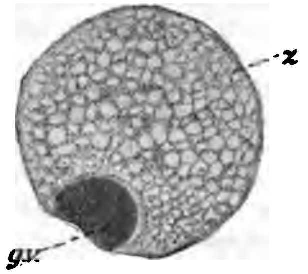


3. *The liberated medusæ.* I have carefully examined the medusæ and free ova that Mr. Duerden collected in the water of his aquarium. The medusæ are so shrivelled and degenerate that nothing of their anatomy could be satisfactorily determined. The ova, however, appear to be in a satisfactory state of preservation, and exhibit one or two features of interest. Each ovum of this series is approximately 0·25 mm. in diameter, that is to say, 0·05 mm. larger in diameter than the ovum in the last stage.

The cytoplasm is very granular and contains numerous vacuoles *and a few zooxanthellæ* (fig. 10, z). The germinal vesicle is very large

and contains no nucleolus and very few or no chromatin granules. Its outline is very ill-defined. The presence of zooxanthellæ in the ovum at this stage is of interest, and it is a matter for regret that I have been unable to determine the manner in which they pass into the cytoplasm from the endoderm of the medusa.

FIG. 10.



In general characters the free ovum of Millepora has a remarkable resemblance to the ova of Alcyonium and other Alcyonarians, and I believe that this points to the conclusion that its specific gravity is very slightly higher than that of sea water, and sinks extremely slowly when set free.

We are indebted to Mr. Duerden (1) for a brief account of these medusæ as observed in an aquarium (see figs. 4—9). "When first liberated their walls appeared quite wrinkled, and the interior was occupied with three or four opaque bodies (the ova). No stalked tentacles were developed, their place being taken by four or sometimes five swollen areas, where a few large stinging cells were located. The medusæ were very sluggish in their movements, feebly pulsating only now and again. While under observation a curious action began to take place; the opaque bodies in the interior were seen to be extended through the mouth of the medusæ" (*i.e.*, the mouth of the umbrella cavity), "sometimes singly, sometimes two or more partly connected. These became spherical, and appeared to have a slight movement of their own. Having discharged themselves in this way, the medusæ shrunk up and their mission was apparently ended. The whole process, liberation of the medusæ and extrusion of the spheroidal bodies, was completed in five or six hours."

There are almost insuperable difficulties in the way of maintaining a sea-water aquarium in the tropics in a thoroughly healthy condition. I am satisfied that Mr. Duerden took every possible precaution, but nevertheless it is probable that in some respects the conditions cannot be relied upon as being strictly normal. The observations, however, are of importance, the structure of medusa as it is seen in the ampulla

suggesting that it is not capable of living a long time free from the colony, and the character of the cytoplasm indicating that when the ovum is set free it is very buoyant, and is probably fertilised and passes through the early stages of its development suspended in the water.

It is almost certain that the medusa does not digest food and nourish the ova after its escape from the ampulla, and I am inclined to believe that after a few pulsations which are sufficient to carry it away from the region of the colony, the ova are set free and the medusa dies.

Correction.—In a former communication to the Royal Society (4), I described certain cells in the cœnosarc of Millepore from Celebes as ova. Since the discovery of the female medusa, I have carefully re-examined my preparations, and satisfied myself that I made a mistake. These cells are not ova, but the cells which ultimately give rise to the large kind of nematocyst.

LITERATURE.

1. Duerden, J. E. "Zoophyte collecting in Bluefields Bay," 'Journal of the Institute of Jamaica,' March, 1899.
2. Hickson, S. J. "The Medusæ of *Millepora Murrayi*," 'Quart. Journ. Micros. Sci.,' 1891.
3. —— "Notes on the Collection of Specimens of the genus Millepora, obtained by Mr. Stanley Gardiner at Funafuti," 'Zool. Soc. Proc.,' 1898.
4. —— "The Sexual Cells and Early Stages of Development of *Millepora plicata*,' 'Phil. Trans.,' B, vol. 179.

"Vapour-density of Bromine at High Temperatures." By E. P. Perman, D.Sc., and G. A. S. Atkinson, B.Sc. Communicated by Professor Ramsay, F.R.S. Received November 14,—Read December 7, 1899.

It has been proved by one of us in a previous paper* that the vapour-density of bromine is normal up to a temperature of 279° C., a result in entire opposition to the numbers obtained by J. J. Thomson.†

We have now determined the densities at temperatures ranging from about 600° C. to 1050° C. by a modification of the former method, the chief difference being that the globe was not filled with the vapour by boiling out the excess of liquid, as in the usual method, but by admitting the bromine, already in the form of vapour, to the globe which remained in the furnace throughout the experiment.

Apparatus used.—A is a porcelain globe (fig. 1), in some experiments

* 'Roy. Soc. Proc.,' vol. 48, p. 45.

† 'Roy. Soc. Proc.,' vol. 42, p. 345.

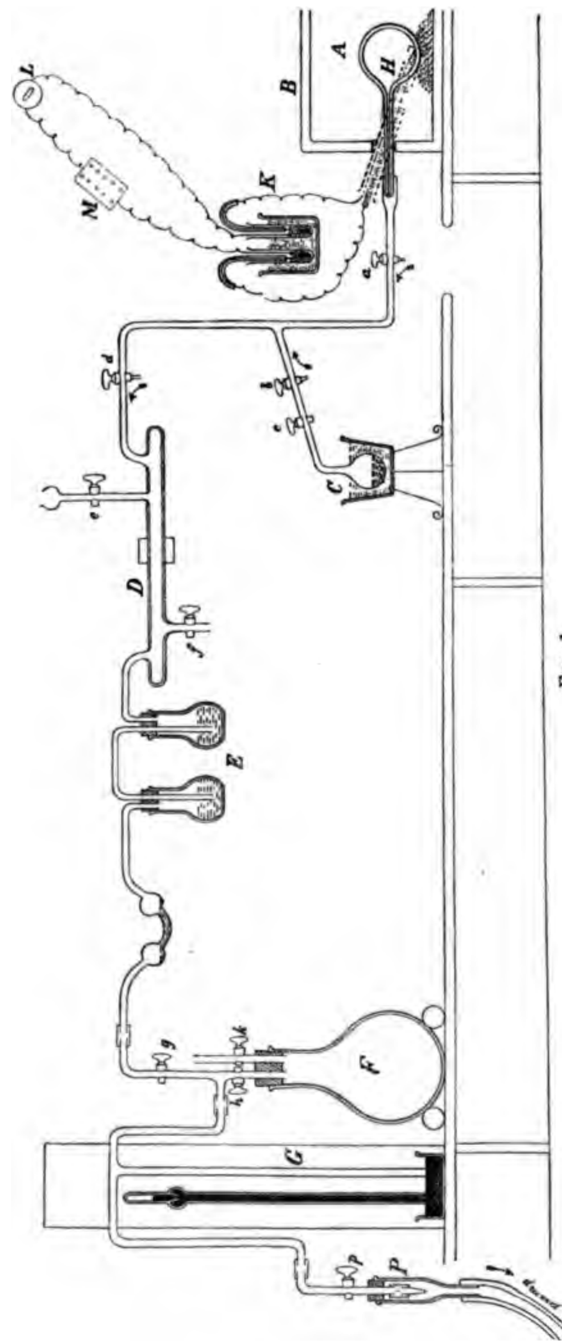


Fig. 1.

of a capacity of about a litre, in others of about 250 c.c.; its stem, of about 1 mm. bore, was cemented by means of a mixture of litharge and glycerine, to a cup on the capillary tube carrying *a*; B a muffle furnace, C a glass globe of about 200 c.c. capacity, placed in a water-bath; D and E absorption apparatus; F a large globe of about 8 litres capacity for steadying the pressure; G a pressure gauge; H a thermo-electric couple of platinum and platinum-rhodium enclosed in a porcelain tube; K the "cold junction," each wire of the couple being connected with a copper wire and placed in a test-tube containing alcohol, the two tubes standing in a beaker of water; L the galvanometer, which, with M, the resistance box and switch, and N, the scale and lamp, were in an adjoining room. P is a Bunsen valve to prevent back-rush of water from the pump; *a*, *b*, *c*, *d*, *e*, *f*, *g*, *h*, *k*, and *l* are stopcocks, *a*, *b*, and *d* being diagonal three-way stopcocks arranged so that air could be admitted from the outside in the directions shown by the arrows. These stopcocks were obtained from Messrs. C. E. Müller and Co., and have proved extremely satisfactory. The lubricant used on *a*, *b*, and *c*, which came into contact with liquid bromine, was phosphorus pentoxide which had become viscous by exposure to the air. Further action of the air was prevented by a covering of burnt rubber and vaseline.

Method of Procedure.

Filling the Globe.—The small globe C containing bromine was heated by means of the water-bath to about 60° C., *i.e.*, about the boiling point of bromine under atmospheric pressure. The globe A was exhausted by means of a water air pump, through the connections shown, to about one-sixth of an atmosphere, sometimes less. The stopcock *d* was then closed, and *b* and *c* opened to admit bromine into the globe A, the connecting tubes being warmed by means of a Bunsen burner to prevent the condensation of bromine in them. After a short interval (15 to 30 secs.) *b* was closed and *d* opened; the globe was exhausted to about the same extent as before, and all the bromine carried off was absorbed by strong caustic soda solution placed in the absorption tube and flasks D and E. (The arrangement of the flasks shown in the figure was found to be a very convenient one; it consists of two small wash-bottles with the long tubes connected, or in one piece; by this arrangement the liquid was never carried over in either direction.)

The large globe F was usually kept exhausted and the pump at work. If necessary the globe A could be connected directly with the pump by turning off stopcock *h*.

The globe was again put into connection with C, again exhausted, and so on, the filling process being repeated seven times. Some bro-

mine was then allowed to condense above the stopcock *a*, and the stopcock *b* was opened to the air; *a* was then turned on, when usually a little bromine entered, and finally began to blow out on reaching the hot neck of the globe; *a* was then turned off, and the tubes between *a*, *b*, and *d* cleared of bromine by a current of air; *a* was again turned on for a few seconds, and the tubes cleared again; this was repeated until no more bromine was seen to come out on opening the stopcock. During this process the neck of the globe and the stopcock were kept warm by heating with a Bunsen burner; considerable difficulty was sometimes experienced in getting the stopcock free from bromine.

The globe was thus filled with bromine vapour at the atmospheric pressure and the temperature of the furnace; the latter was read on the galvanometer scale at the moment of opening the stopcock *a* for the last time. The stopcock *c* was then closed, and the bromine removed from the tube *bc* by repeated exhaustion. The bromine in the globe C was thus cut off from the rest of the apparatus by the exhausted space *bc*.

Absorption and Estimation of the Bromine.—A strong solution of potassium iodide was placed in the tube and flasks D and E, and the globe A was exhausted to about one-sixth atmosphere as in the filling, the bromine drawn off being caught by the potassium iodide. Air was then admitted to the globe through *b*, and the exhaustion repeated.

After the seventh exhaustion the tube was washed out through the stopcocks *e* and *f*, and the solution added to that from the flasks E, and titrated with a standard sodium thiosulphate solution of about N/5 strength. The solution was standardised by means of pure bromine weighed in small bulbs and dissolved in potassium iodide solution. When a series of determinations was made at different pressures, the globe was filled in the usual way, and then slowly exhausted to the required amount, the bromine drawn off being absorbed by potassium iodide solution as usual, except that the absorption flasks were replaced by a straight tube with two small bulbs blown in it containing some of the solution; this arrangement was to obviate the back pressure caused by the solution in the flasks.

The residual amount of bromine was drawn off as before described.

Determination of Temperature.—The thermo-electric couple was standardised by means of boiling selenium and solidifying potassium sulphate. The latter was melted in a small porcelain crucible by means of the oxygen-gas flame; the porcelain was not appreciably attacked if the operation was conducted quickly. The galvanometer, a dead-beat d'Arsonval, was obtained from Messrs. Nalder Bros., and had a resistance of about 200 ohms; the focal length of the mirror was 40 inches. In order to bring the zero point and the reading for the highest temperature employed upon the scale, it was necessary to introduce a resistance of 11,000 ohms, which remained in the circuit

during the whole of the experiments. With this arrangement one scale division was equivalent to about 2° C. The porcelain tube containing the couple was supported by asbestos some distance above the floor of the muffle. The variation of temperature between the floor and the top of the muffle was found to be 6° C. at a temperature of about 600° C., and 8° C. at 900° C.; no correction was made for this difference as the couple was near the level of the centre of the globe.

Between the two fixed points obtained the scale reading was taken to be proportional to the difference of temperature, according to the method used by Roberts-Austen.

Determination of Pressure.—The pressure was read off on a standard barometer, or on the pressure gauge G in the case of pressures less than atmospheric.

Capacity of the Globe.—The porcelain globe was weighed full of air, and again weighed, filled with water, and the capacity calculated. The filling with water was effected by repeated exhaustion and letting in water.

Preparation of Pure Bromine.—Commercial bromine was purified by one of the processes previously employed, viz., by boiling with potassium bromide, distilling over red-hot manganese dioxide, shaking with strong sulphuric acid, and re-distilling. The greater portion came over at a perfectly constant temperature, 58·9° C. at a pressure of 761 mm. (*Cf.* former paper.)

Results.

Series I. About 650° C.

Weight of Br. grammes.	Volume of globe. c.c.	Pressure. mm.	Temperature.	Vapour-density.
0·6076	294·3	765·5	677·5°	79·3
0·6348	294·3	765·5	650·0	80·4
0·6013	275·1	758·0	631·0	80·4
			Mean.........	80·0

Series II. About 830° C.

Weight of Br. grammes.	Volume of globe. c.c.	Pressure. mm.	Temperature.	Vapour-density.
0·4914	276·0	758·0	828·0°	79·7
0·4656	276·0	758·0	873·0	78·6
0·4850	276·0	758·0	833·0	79·0
0·4896	276·0	758·0	830·0	79·6
1·9380	1052·0	767·0	799·0	79·6
0·5335	295·1	765·0	833·0	80·6
0·5371	295·1	765·0	820·0	80·1
0·5244	295·1	765·0	845·0	80·0
			Mean.........	79·7

Series III. About 900° C.

Weight of Br. grammes.	Volume of globe. c.c.	Pressure. mm.	Temperature.	Vapour-density.
1·7440	1053·0	767·0	903·0°	78·5
0·8340	1053·0	365·5	901·5	78·7
			Mean.........	78·6

Series IV. About 950° C.

1·6400	1053·0	764·0	956·0°	77·4
1·6490	1053·0	764·0	951·0	77·6
			Mean.........	77·5

Series V. About 1015° C.

0·4003	276·5	757·0	1016·0°	75·9
0·0275	276·5	55·75	1010·0	70·4
0·4099	276·5	757·0	1012·0	77·4
0·0302	276·5	60·0	1016·0	72·3
0·4078	276·5	757·0	1022·0	77·6
0·03195	276·5	62·7	1016·0	73·1
0·3982	276·5	755·0	1016·0	75·8
Mean of vapour-densities at atmospheric pressure ...				76·7

Series VI. About 1050° C.

0·3828	276·6	760·0	1057·0°	74·5
0·3860	276·6	760·0	1055·0	75·0
0·3771	276·6	760·0	1055·0	73·3
0·3891	276·6	760·0	1057·0	75·7
0·3773	276·6	760·0	1057·0	73·5
1·4390	1055·0	763·0	1055·0	73·5
1·4680	1055·0	764·0	1034·0	73·7*
			Mean.........	74·3

Series VII. About 1040° C., varying Pressure.

0·3943	276·6	755·0	1035·0°	76·0
0·1615	276·6	319·3	1039·0	73·9
0·3915	276·6	755·0	1041·0°	75·8
0·1608	276·6	318·5	1042·0	73·9
0·0947	276·6	188·7	1041·0	73·3
0·02323	276·6	47·3	1041·0	71·8

* This number is not included in taking the mean, as the temperature is considerably below the others.

Chief Possible Sources of Error.

(1) *Error in filling and emptying Globe.*—To avoid error in filling, the globe was exhausted and filled with bromine seven times, as mentioned above. Each exhaustion being to at least one-sixth atmosphere, the volume of the residual air would be theoretically $\left(\frac{1}{6}\right)^7$, *i.e.*, less than 1/250,000 of the volume of the globe. Similar remarks apply to the emptying.

As a practical test, a glass globe was filled in the manner described, and on opening it under water, only a negligible amount of air was found to be present.

(2) *The Low Temperature of the Stem.*—An approximate correction for this was made in the following manner :—

Let v = volume of globe.
δ = vapour-density of bromine in globe.
T = absolute temperature of globe.
v', δ', T$'$ = corresponding quantities for the stem.
D = apparent vapour-density.

Then $$\delta = D - \frac{v'}{v}\left(\frac{\delta' T}{T'} - D\right).$$

The maximum correction thus calculated is only 0·4.

(3) *Loss of Bromine by Incomplete Absorption.*—The potassium iodide solution in the small bulbs of the absorption apparatus E remained uncoloured, showing that the absorption was practically complete.

(4) *Leakage at the Stopcocks.*—In the earlier experiments ordinary stopcocks were used, and were found to leak to such an extent as to be useless for the purpose. Well-made diagonal stopcocks were then employed and were found quite trustworthy.

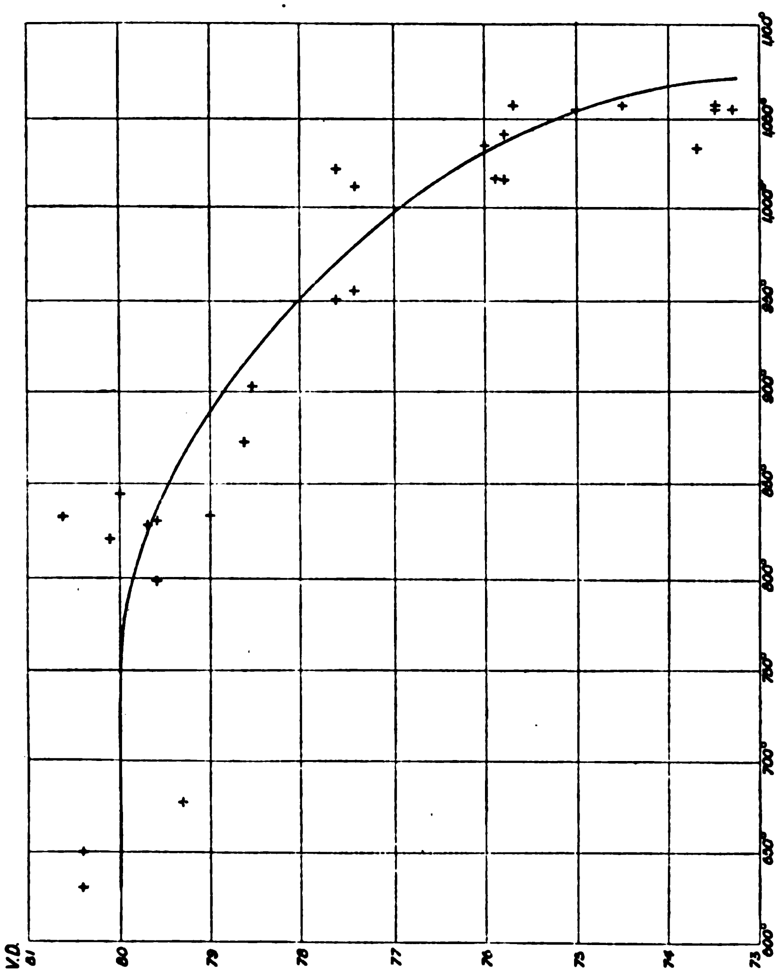
(5) *Error in Temperature Determinations.*—It is very difficult to say what the maximum error is; we think it to be not more than 10°, if so much. The chief source of uncertainty was the drift of the zero of the galvanometer; this was reduced to a minimum by switching in just before taking a reading.

(6) *Expansion of the Porcelain Globe.*—A correction was made for this, the coefficient of expansion found by Deville and Troost, 0·0000108, being used. This gives practically the same result at the temperatures considered as the formula of T. G. Bedford (British Association, 1899).

General Conclusions.—The results at atmospheric pressure have been plotted against temperature (see Curve I). From this it appears that the vapour-density of bromine is normal up to about 750° C.; at this point dissociation becomes appreciable and gradually increases with rise of temperature. At 1050° C. the density falls to 75·25.

The constants of dissociation were calculated as follows :—

Taking the fundamental equation $u/u_1^2 = c_1/c$, where u and u_1 are the number of molecules per unit volume of Br_2 and Br_1 respectively,



CURVE I.

and c and c_1 the velocities of dissociation and recombination, we have, after reduction,

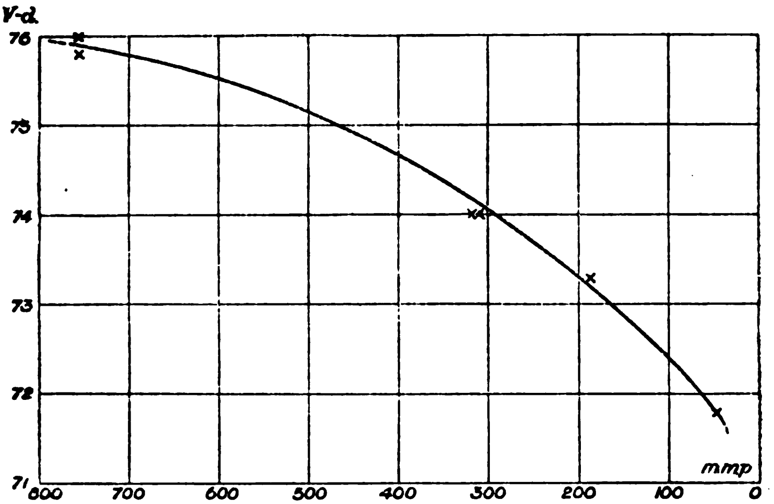
$$\frac{ac_1}{c} = \frac{40T(D - 40)}{P(80 - D)^2} = \gamma \text{ (say)},$$

where a is the number of molecules per unit volume of any gas under standard conditions.

T.	D.	γ.
800°	79·87	13320
850	79·48	8630
900	78·83	1750
950	78·01	620
1000	76·94	264
1050	75·26	110

The above values of T and D were read from the curve.

Series III, V, and VII show the effect of varying the pressure. The results of series VII form a smooth curve, vapour-density being plotted against pressure (Curve II), but the dissociation-constant calculated from them is irregular.



V-d of Bromine under varying pressure. Temp. 1040°.

CURVE II.

The authors regret that the apparatus available did not enable them to extend the experiments to higher temperatures than those employed. In conclusion they wish to thank the Government Grant Committee of the Royal Society for their assistance.

ADDENDUM.

December 8.—It may be interesting to add the results obtained by other observers, and referred to in our previous paper.

Temperature.	Vapour-density.	Observer.	Remarks.
About 1570°...	53	V. Meyer and Züblin	Nascent bromine from $PtBr_4$.
" " ...	80 to 52·6	"	Free bromine.
" " ...	63·4 to 64·7	Crafts	
" (445°)	(75·7 instead of normal)	"	(To show the accuracy attained.)

It should be noted that in all these experiments the bromine was mixed with an indeterminate quantity of air, which makes them hardly comparable with our results.

"*Polytremacis* and the Ancestry of the Helioporidæ." By J. W. Gregory, D.Sc. Communicated by Professor Lankester, F.R.S. Received November 21,—Read December 7, 1899.

(Abstract.)

The recent blue coral *Heliopora* presents striking resemblances in structure to the palæozoic *Heliolites*. All the earlier writers on corals accordingly regarded the two genera as intimately allied. But some later authorities consider the resemblances as accidental, and that the corals have no special affinities. Thus, according to F. Bernard, *Heliopora* and *Heliolites* belong to distinct subphyla. Lindström admits only one species of *Heliopora*, and regards the genus as quite isolated, as essentially distinct in structure from *Heliolites*, and as further separated from the latter by "the total absence of all connecting links from the end of the middle Devonian to the recent times." The author, however, considers that the original view of the close affinity of *Heliopora* and *Heliolites* is correct, that the two genera are essentially similar in structure, and that they are linked by a series of eocene and cretaceous corals. Amongst these fossils is the genus *Polytremacis*, which is redescribed, and a new species of *Heliopora* from the Cretaceous of Somaliland. It is suggested that *Heliopora* has descended from the palæozoic Heliolitidæ by degeneration in size and increase in number of the cœnenchymal cæca.

"Gold-Aluminium Alloys." By C. T. HEYCOCK, F.R.S., and F. H. NEVILLE, F.R.S. Received October 31,—Read December 7, 1899.

(Abstract.)

The first part of this paper gives the equilibrium curve for the liquid alloys and the various solid bodies that can form in them. The curve is based on the determination of the freezing points of mixtures varying in composition from pure gold to pure aluminium.

The freezing points were determined by means of platinum resistance pyrometers of the Callendar-Griffiths type, and the composition of each alloy was found by extracting a sample from the crucible and analysing it.

The ordinate in the curve is the freezing point on the air-centigrade scale, and the abscissa is the composition of the alloy expressed in atomic percentages of aluminium.

The curve was found to consist of seven branches, each branch corresponding to a state in which a particular solid crystallises first. In harmony with this, seven substances can be detected in the solid alloys. The bodies are :—

Gold; Au_4Al; Au_5Al_2 or perhaps Au_8Al_3; Au_2Al; a body which is probably AuAl; $AuAl_2$, Roberts-Austen's purple alloy; aluminium.

The bodies Au_2Al and $AuAl_2$* are indicated by well-marked summits in the curve, at 33·4 and at 66·6 atomic per cents. of aluminium respectively.

The melting or freezing point of Au_2Al is at 625° C., that of $AuAl_2$ is at 1062° C., apparently identical with the melting point of gold itself. The body whose formula we give as either Au_5Al_2 or Au_8Al_3 has its melting point at 575° C. The body Au_4Al has its melting point near 550° C. That of the hypothetical AuAl is not given by the curve.

The curve has three well-marked eutectic angles. One of these is at 527° C. and 3·6 per cent. by weight of aluminium, the alloy here being a mixture of Au_4Al and Au_5Al_2. The next is at 569° C. and 8·36 per cent. by weight of aluminium; this alloy has a composition corresponding to the formula Au_3Al_2, but it is a mixture of Au_2Al and AuAl. The third eutectic is at 648° C., and the alloy contains 1·87 per cent. by weight of gold; it is a mixture of $AuAl_2$ and aluminium. We see from the above that the mixture with the lowest melting point of all is that containing 3·6 per cent. by weight of aluminium, this small percentage depressing the melting point of gold from 1062° C. to 527°, that is more than 500°. A liquid of this composition, though

* See footnote, p. 21.

almost wholly composed of gold, will not begin to solidify until this comparatively low temperature of 527° C. is reached.*

Each of these eutectic points gives rise to a horizontal row of second freezing points in the curve, and the alloys containing more than 44 and less than 60 atomic per cents. of aluminium have three distinct freezing points corresponding to the successive formation of three solid bodies.

The four compounds, Au_4Al, Au_5Al_2, Au_2Al, and the hypothetical AuAl, are pure white substances. $AuAl_2$, as is well known from the work of Sir W. Roberts-Austen, is a magnificent purple body.

The latter part of the paper gives the result of a microscopic examination of polished and etched sections of alloys taken from various parts of the curve. All the bodies referred to above could be distinguished under the microscope. The photomicrographs accompanying the paper show that the structure of the solid alloy is everywhere in strict harmony with the indications of the freezing point curve. Speaking generally, we may say that the patterns observed in the photographs repeat themselves at corresponding points of each branch of the curve. For example, near the summit of the branch corresponding to the pure alloy, Au_2Al, the photograph shows us more or less hexagonal polygons of this substance almost entirely filling the field, and only separated from each other by very fine lines of impurity. If we take a section of an alloy a little way below the summit, we see the polygons of Au_2Al surrounded by a ribbon-like network of mother substance. Still further down, the crystals of Au_2Al are scanty, and arranged in such regular patterns, generally in lines at right angles to each other, as to render it certain that they crystallised freely while surrounded by liquid. Finally, at the bottom of the branch, that is at the eutectic point, the large crystals of Au_2Al are absent, and the whole field is full of the mother substance, which is sometimes but, as we explain in the paper, not always a eutectic mixture.

If, leaving the eutectic point, we ascend the next branch, these phenomena repeat themselves, but the primary crystallisation (that is the matter which solidified first) is now of a different substance.

Some of the photographs of alloys very rich in aluminium were taken by the Röntgen rays, and an enlargement made from the negative. The contrast between the Röntgen ray photograph and the surface photograph of the same alloy shows what a much better picture of the structure of the alloy is given by the Röntgen rays.

* The rapid depression in the freezing point of gold, due to the presence of small quantities of aluminium, and the great rise in the freezing point as the composition corresponding to the compound $AuAl_2$ is approached, have been already discovered by Sir William Roberts-Austen.

"On the Association of Attributes in Statistics, with Examples from the Material of the Childhood Society, &c." By G. UDNY YULE, formerly Assistant Professor of Applied Mathematics, University College, London. Communicated by KARL PEARSON, F.R.S. Received October 20,—Read December 7, 1899.

(Abstract.)

The paper deals with the theory of association of attributes, *i.e.*, invariable attributes, as opposed to the "correlation" of variables. Two attributes A and B are independent or unassociated if

$$(AB) = (A)(B)/N,$$

(A) being the frequency of the attribute A, (B) that of B, and (AB) the frequency of the pair AB; N being the total number of observations. If this relation do not hold, they are "associated."

Section (I) of the paper is introductory, describing the subject-matter and notation, which is essentially that of Jevons.* Calling a group defined by n attributes ABCD......N an nth-order group, Section (II) deals with the fundamental problem of the number of independent nth order frequencies that can be formed from m attributes; *i.e.*, the number of such frequencies that must be given before the remaining frequencies of the same order can be calculated. Certain extremely curious relations are shown to hold in the special case of "equality of contraries," where all pairs of contrary frequencies (A) (α), (AB) ($\alpha\beta$), (ABC) ($\alpha\beta\gamma$) are equal, α being the contrary of A—*i.e.* not A—and so on.

Section (III) proceeds to the theory of association proper. The function

$$Q = \frac{(AB)(\alpha\beta) - (A\beta)(\alpha B)}{(AB)(\alpha\beta) + (A\beta)(\alpha B)}$$

is proposed as a "coefficient of association." It is zero when the attributes are independent, $+1$ when all A's are B or all B's are A, and -1 when all A's are β or all β's are A, and thus measures the approach towards "perfect association" in the same sort of way as the correlation coefficient measures the approach towards perfect correlation. The connection between correlation and association is touched upon, and it is pointed out that one may form "partial" coefficients of association (by limiting the extent of the universe of

* "On a General System of Numerically Definite Reasoning," 'Manchester Lit. and Phil. Soc.,' *1870*, and "Pure Logic and other minor works" p. 173.

discourse) roughly corresponding in their uses to partial coefficients of correlation.

In Section (IV), the values of the Probable Errors, and the correlations of the errors in the chief constants, are obtained. The probable error of Q is

$$0{\cdot}6745\,\frac{1-Q^2}{2}\sqrt{\frac{1}{(AB)}+\frac{1}{(A\beta)}+\frac{1}{(\alpha B)}+\frac{1}{(\alpha\beta)}}\,.$$

In Section (V), a series of miscellaneous illustrations are given (association of smallpox attack rate and non-vaccination; association between temper of husband and wife, inheritance of artistic faculty &c., from Mr. Francis Galton's 'Natural Inheritance'; association between vigour of offspring and crossing of parentage in plants from Darwin's 'Cross and Self-fertilisation').

In Section (VI), the "Association of defects in children and adults," is treated more at length as an example of the methods advocated, the material being drawn mainly from the Report of the Committee on the Scientific Study of Childhood. It is shown that the association coefficient is almost uniformly higher for women than men, and for children than adults. This last effect is however a mixed one, due partly to selection, partly to change in the individual, and the material available does not enable us to separate the partial effects. These two laws of association appear to correspond to similar ones for correlation; women being more highly correlated than men, and children than adults.

"Data for the Problem of Evolution in Man. III.—On the Magnitude of certain Coefficients of Correlation in Man, &c." By KARL PEARSON, F.R.S., University College, London. Received November 20,—Read December 7, 1899.

1. This paper contains a number of data bearing on the correlation of characters, &c., in man which have been worked out by my collaborators during the last few years, and several of which seem of considerable importance for problems relating to the evolution of man. In each case the data were procured or reduced with a view to answering some problem which had directly arisen during our inquiries as to the action of natural selection on man. Questions as to the alteration of correlation with growth or the influence of homogamy on fertility demand definite answers before the general theory of the influence of natural selection on a growing and reproductive population can be effectively developed.

(A).—*On a Monthly Period in the Birth-rate.*

2. While it is well known that there is an annual period in the birth-rate, no attempt, as far as I am aware, has been made to ascertain whether a lunar period exists. Accordingly, I applied to Dr. E. C. Perry, superintendent of Guy's Hospital, who most kindly placed at my disposal the maternity records of that charity. As preparatory work Mr. Yule and I extracted upwards of 6,000 births with their dates. These, with the assistance of Mr. L. Bramley-Moore, we arranged in four groups, male and female, and in twenty-nine and thirty day lunar months as given by the almanack of the year. In each lunar month the number of births on each successive day following the new moon was tabulated, and each month was then reduced to the same total, so that no month might be weighted by its relation to the annual variation in birth-rate. Thus four curves were obtained, each embracing the material for twenty-four months, and giving the daily fluctuation in male and female birth-rate for twenty-nine and thirty day lunar months. In none of these curves was there any *significant* deviation from the diurnal average on any day. The curves were then harmonically analysed by Mr. Yule; the result gave no approach to agreement between the amplitudes or phases in the four cases. Had there been any approach we should have gone on to 20,000 births as we originally proposed, but it seemed merely a waste of labour. I conclude, therefore, that if there be any monthly period in the birth-rate, it is of very small importance. There is little or no correlation between lunar phase and birth frequency. The object of this inquiry was the following: The average regular recurrence of the monthly period in woman has been taken to suggest a tidal influence on the primitive ancestry of mankind;* there is an indisputable correlation between birth and the date at which a monthly period would have taken place had pregnancy not intervened. Hence a positive result might have confirmed this suggestion of tidal influence, as well as explained a certain amount of folklore connecting birth and lunar influence.

Our negative result merely shows that if lunar or tidal influence ever fixed the period of the menses, sensible correlation between the two has now disappeared.

(B).—*On the Correlation between Weight and Length of Infants at Birth.*

3. A table of the length and weight of infants at birth was given in a "Report of the Anthropometic Committee of the British Association"

* "In the lunar or weekly recurrent periods of some of our functions we apparently still retain traces of our primordial birthplace, a shore washed by the tides," Charles Darwin, 'The Descent of Man,' p. 161, 2nd ed.

I.—Correlation bet

	6	
2–16	0–4	4-8
)·5		
l·0	0·5	0·5
3·0	9·75	5·25
3·75	34·25	22·75
3·75	32·5	19·5
3·0	6·0	11·0
)·5	2·0	1·5
·5	85·0	60·5

L = length
P_L = proba
P_W = prob

in., and the probable
reight by two to three

II.—Correlation b

6 lbs.		
4–8	8–12	1
	1·0	
1·0	1·25	
24·0	15·5	1
21·0	40·5	3
6·25	14·75	2
1·75	3·0	
54·0	76·0	7

L = lengt
P_L = prol
P_W = pro

ation of the array co
r suspecting that it

for 1883, and is published in the 'B. A. Transactions' for that year, p. 286. The measurements were comparatively few in number (450 for each sex) and were made partly in London and partly in Edinburgh. Accordingly I thought it better to obtain new material for calculating correlation. Through the courtesy of Dr. J. D. Rawlings I was able to obtain copies of the measurements made on between 2000 and 3000 new born infants at the Lambeth Lying-In Hospital. From these 1000 male and 1000 female babies born at the normal period were taken, twins being excluded. The correlation tables and the calculatian of the variation and correlation constants are due to Mr. L. Bramley-Moore. The correlation tables seem of such importance for medical and other purposes that they are given below (see Tables II and III). The following table contains a summary of results.

I.—Weight and Length of New-born Infants. 1000 of each Sex.

	Mean.		Standard deviation.		Coefficient of variation.		Coefficient of correlation.
	Weight.	Length.	Weight.	Length.	Weight.	Length.	Weight with Length.
	lbs.	ins.	lbs.	ins.			
Females ...	7·073 ±0·021	20·124 ± 0·025	1·006 ±0·015	1·177 ±0·018	14·228	5·849	0·622 ±0·013
Males......	7·301 ±0·024	20·503 ± 0·028	1·144 ±0·017	1·332 ±0·020	15·664	6·500	0·644 ±0·012

Table I confirms the view already expressed by me, that the male infant is at birth more variable than the female: that female infants from six to ten years of age are more variable than male in both weight and height, appears to be not a result of selection but of growth.* Both sexes lose not only variability but correlation as they grow older, and this is a fundamental point to be borne in mind, when attempts are made to trace the influence of natural selection in the change of variation or correlation in a group of *growing* animals. Even the co-efficient of variation, which is far more stable than the standard deviation (or absolute variability) is seen to alter considerably with growth. So far as weight and height are concerned female and male both lose variation as they grow older, but women less rapidly than men. So far as correlation between weight and height is con-

* See "Variation in Man and Woman," 'The Chances of Death,' vol. I, *pp. 296, 307, and 308—309.*

cerned, men start with a scarcely sensible advantage over women as infants, and conclude as adults with an immensely less correlation than women, among whom it appears to have slightly increased, or at any rate not to have decreased. Until we have accurate numerical determinations of the change in the correlation between organs with growth, it is impossible to attempt to measure quantitatively the influence of a selective death-rate on growing living forms. We can only deal with the influence of sudden selection on growing organisms, or of long continued selection on adult life.

(C).—*On the Correlation between Stature, Weight, Strength, and Head Index in the Case of Adults.*

4. The measurements upon which these results are based were taken from cards in the possession of the Cambridge Anthropometrical Committee, who kindly allowed me to have copies taken for 1000 cases of male students, and for the whole series of female students, which unfortunately were only about 160 in number.* The whole of the lengthy arithmetic involved in the calculation of the constants was undertaken by Miss C. D. Fawcett, B.Sc. The bulk of the students were between nineteen and twenty-five years of age, although some few were older; they may be taken to represent adults, who in the great majority of cases were in good physical health and training, and were not troubled with the superfluous weight of a later period of life.

I will first put in a separate table the results for weight and height, in order that the constants for adults can be easily compared with those for new-born infants.

IV.—Weight and Height of Adults. 1000 Males, 160 Females.

	Mean.		Standard deviation.		Coefficient of variation.		Coefficient of correlation.
	Weight.	Height.	Weight.	Height.	Weight.	Height.	Weight with Height.
	lbs.	ins.	lbs.	ins.			
Females....	125·605 ±0·773	63·883 ±0·130	14·030 ±0·546	2·361 ±0·092	11·170	3·696	0·721 ±0·026
Males......	152·784 ±0·353	68·863 ±0·054	16·547 ±0·250	2·522 ±0·048	10·830	3·662	0·486 ±0·016

* *The establishment of* an anthropometric laboratory at Newnham College will *soon increase this total.*

This table shows us the decrease of variability with age in both weight and height. But the female is now more variable than the male. Were this change to be attributed to natural selection, with a stronger incidence on the male than the female, then we have the anomaly that the correlation has been reduced in the male, but increased in the female, while theoretical considerations would lead us to the conclusion that it ought to be reduced in both. We are compelled to consider the changes in variation and correlation as due to growth or nurture; or, if there be selection, it is to a large extent screened by these causes.

V.—Correlation between Height, Weight, and Strength of Pull. (Data for 1000 Males and 160 Females.)

	Organs.	Coefficient of correlation.
Females ..	Strength of pull and height	0·216 ± 0·052
Males	Strength of pull and height	0·303 ± 0·019
Females ..	Strength of pull and weight	0·338 ± 0·049
Males	Strength of pull and weight	0·545 ± 0·015

I expect the greater correlation of the male in these two cases is due to the fact that a better physical training has taught him how to make use of his height and weight, especially the latter, in exerting his strength.

VI.—Correlation between Height, Weight, Strength of Pull, and Head Index. (Data for 1000 Males only.)

Organs.	Coefficient of correlation.
Height and head index........	−0·082 ± 0·021
Weight and head index.......	0·011 ± 0·021
Strength of pull and head index	0·041 ± 0·021

Thus it is only in the first case that the correlation with head index can be considered as significant, and in this case it is *negative*. There is no reason, then, for supposing brachycephalic persons stronger or heavier than dolichocephalic, but they do appear to be slightly *shorter*. *We conclude*, therefore, that dolichocephalic persons (? races)

also will be found to be taller than the brachycephalic. Here we have dealt with the correlation between *shape* of head and physique, the correlation between absolute size of head and physique will be given later. It would be of the greatest value to obtain the intensity of correlation between shape of head and intellectual capacity. We hope to return to this on another occasion.*

5. We may place here the variation results for pull and head index.

VII.

Sex.	Organ.	Mean.	Standard deviation.	Coefficient of variation.
Female ...	pull in lbs.	49·220±0·452	8·217±0·320	16·69
Male	pull in lbs.	84·016±0·270	12·676±0·191	15·09
Male	head index	79·572±0·064	2·999±0·044	3·77

(D).—*On the Correlation of Fertility with Homogamy.*

6. In the reduction of my family measurements, I have been much struck by the very high values obtained for the correlation in characters between husband and wife. For 1000 cases in which the stature of husband and wife were determined, the correlation was nearly 0·3! This is almost as close a resemblance as we have found for some characters between father and daughter. Now there is little doubt that there is a certain amount of conscious assortative mating in this respect; a short man does not, as a rule, like a very tall wife. There is further an unconscious mating arising from neighbours marrying; neighbours in England often mean persons of the same local race, and such local races differ considerably in their mean statures.† But my data were very largely drawn from the professional classes, and in large towns like London to marry "in the set" hardly means to marry into the same local race. The parents, however, may in some cases have come to London from the same locality. The question whether husband and wife spring from the same rural district is one of considerable interest, and deserves special investigation. I hardly believe, however, that it will be found a source contributing much to the intensity of assortative mating in my own data. A large proportion of the data cards were filled in by members of the London professional

* Measurements are now being made on brothers and sisters in schools, the apparatus being provided by aid from the Government Grant. It will, however, be some years before sufficient data have been collected.

† *Mr. Francis* Galton has pointed out this source of indirect assortative mating *to me as worthy of consideration.*

classes, and marrying within the district or set cannot, I think, introduce much local race influence.

I accordingly turned the problem round, and asked if there appeared any reason why, in collecting my material, I should have selected unconsciously homogamous marriages. Now, clearly, I was much more likely to get a return from a large than a small family; *one* member of a family of eight was more likely to take interest in the matter than one member of a family of two or three. It seemed to me that out of 10,000 families I was clearly more likely to get returns from the larger families than the smaller ones, remembering that 26 per cent. of the families correspond to 50 per cent. of the offspring. Hence arose the important problem is fertility associated with homogamy? When like mates with like, is the number of progeny greater than when like and unlike mate? Put in this way the problem appears to be of first class importance for the theory of evolution. If homogamy rather than heterogamy results in fertility, then we get a first gleam of light on what may be ultimately of vital significance for the differentiation of species. When any form of life breaks up into two groups under the influence of natural selection, what is to prevent them intercrossing, and so destroying the differentiation at each fresh reproductive stage? Various hypotheses—isolation, recognition marks, physiological selection—have been propounded. But if like mating with like connotes greater fertility, the answer to the problem of differentiation would be simply summed up in differential fertility. We should have merely a case of genetic selection arising from the correlation of fertility and homogamy.

We must be careful, however, not to rush to any conclusions without ample data. In particular we must not confuse homogamy with endogamy. Nor must we argue that relatives being closely alike, kin-marriages would mean increased fertility. Darwin has shown statistically that, as a rule, self-fertilised flowers are more sterile than cross-fertilised; kinship, sameness of stock, means likeness of characters, but likeness of characters does not necessarily indicate sameness of stock. It is quite possible for like individuals of different stocks to be fertile *inter se*, and like individuals of the same stock to be in part or wholly sterile *inter se*. In fact, that homogamy means fertility may in all or certain forms of life be dominated by a more potent rule, namely, that endogamy means sterility. The two statements are not contradictory if we interpret homogamy to mean the mating of two like individuals, not in the first place like because they come of the same stock. In fact, if a man seeks a wife of stature corresponding to his own, he will as a rule have a larger field of suitable mates in the general population than within his own limited kin. Bearing this point in mind, I now turn to the somewhat narrow data available at present for the influence of homogamy in the matter of stature on fertility in man.

7. Taking 205 marriages in which I had details of the stature of husbands and wives and the size of their families, two correlation tables were formed (i) for husbands and wives, as such, (ii) for fathers and mothers, *i.e.*, for husbands and wives weighted with their fertility. The tabulation and calculations for (ii) are due to Mr. L. Bramley-Moore, with the assistance of Mr. K. Tressler. For (i) I had worked out the data some years ago myself. The following results were obtained:—

VIII.—Correlation between between Statures of Husband and Wife.

	No. of cases.	Value of correlation.
Husband and wife....	205	0·0931 ± 0·0467
Father and mother...	965	0·1783 ± 0·0210

Now those results seem at first sight significant. We have practically *doubled* the intensity of assortative mating by weighting the observations with fertility. Fertility would thus not be distributed at random, but would increase with the amount of homogamy. The process of collecting the original data here conceived was totally different from that of my own family data cards, the influence of size of family on chance of procuring data being I consider nothing like as marked.* This is, I think, the source of the difference in correlation of stature between husband and wife being so reduced.

8. In order to further investigate the matter directly, a correlation table was prepared for me by Mr. L. Bramley-Moore, in which the variables were (i) difference in stature of husbands and wives, and (ii) size of family. In this case the statures of the wives were reduced to male equivalents before the difference was taken.† Thus the difference is zero, when the wife has the female stature which corresponds to her husband's. The calculations on this table were made by Miss Alice Lee, D.Sc. The correlation was found to be *negative*, and its value

$$-0{\cdot}1201 \pm 0{\cdot}0464$$

Thus it would seem that large difference in stature means small fertility. But there is danger of a fallacy here, which requires careful investigation. The regression equation for size of family (f) in terms of difference of stature of husband and wife in inches (d) is

* 'Phil. Trans.,' A, vol. 187, p. 269. They were not a selection of families by size, but rather by the existence of fairly complete ancestral records.

† The ratio of mean statures = 1·08 about, and $\frac{1}{13}$ was added to the female stature to convert it into its male equivalent.

$$f = 4{\cdot}7 - \frac{1}{10}d.$$

Thus, since the range of difference is from about -10 to $+10$ inches, we have a fertility varying from 5·7 to about 3·7, or about 42 to 43 per cent. variation in fertility as we pass from wives relatively 10 inches taller than their husbands to wives relatively 10 inches shorter! In other words, our homogamous influence is really cloaked by the fact that big husbands and small wives have for extreme cases some 42 per cent. less offspring than small husbands and big wives, a result of considerable interest from the standpoint of genetic selection, and possibly capable of easy physiological explanation, if pelvic measurements are closely correlated with stature.

In order to disentangle the two factors, I divided up my 205 pairs into the quartile groups, and found the following results:—

IX.

Quartiles.	Range of difference.	Total offspring.	Mean size of family.
1st.......	$-9{\cdot}5''$ to $-2{\cdot}25''$	258·5	$5{\cdot}04 \pm 0{\cdot}26$
2nd......	$-2{\cdot}25''$ to $+0{\cdot}417''$	260·333	$5{\cdot}08 \pm 0{\cdot}26$
3rd.......	$+0{\cdot}417''$ to $+2{\cdot}583''$	229·083	$4{\cdot}47 \pm 0{\cdot}26$
4th.......	$+2{\cdot}583''$ to $+11{\cdot}0''$	215·083	$4{\cdot}20 \pm 0{\cdot}26$

The total number of offspring was 963, or the number to be expected in each quartile 240·75, while the average size of the family is 4·6976, with a standard deviation of 2·7826. These results show us that very nearly half the marriages occur with the wife relatively taller than the husband, and that such marriages give 54 per cent. of the total offspring as against 46 per cent. produced wnen the husband is relatively taller than the wife. The mean family with mother relatively taller than father is $5{\cdot}06 \pm 0{\cdot}18$, and that with father relatively taller than mother, $4{\cdot}33 \pm 0{\cdot}18$, a difference which may be taken as significant.

Grouping the 1st and 4th and the 2nd and 3rd quartiles together, we have for the mean family when husband and wife differ considerably $4{\cdot}62 \pm 0{\cdot}18$, and for the mean family when they differ but little $4{\cdot}77 \pm 0{\cdot}18$. This difference in itself, however, unlike that recorded by the previous process of investigation by weighting with fertility, would hardly be sufficient to demonstrate a correlation between fertility and homogamy.

I accordingly made out a fourth correlation table, in which the variables tabulated were difference of relative statures of husband and wife, without regard to its sign and size of family. The mean relative

difference in stature was found to be 2·751 inches, and the standard deviation of its distribution 2·070 inches. The correlation between difference in stature and size of family was $-0·0236$, or greater fertility appears associated with small differences. The observations, however, are so few (205) that the probable error of the correlation is 0·0471, and thus no stress can be laid on this result. If the reader asks why is not the result in § 7 conclusive, the answer must be, it would be conclusive, if the means of the husbands and wives weighted with their fertility were the same as when they were unweighted; increased correlation would then necessarily connote that fertility was associated with homogamy. Actually the fact that absolutely taller mothers are the more fertile alters the centre of the correlation table, and somewhat obscures the issue as to whether the whole increase of correlation is really due to homogamy being correlated with fertility.

That in man, whether from conscious or unconscious sexual selection, there is far more homogamy than has hitherto been supposed, my family data cards amply demonstrate. If in man, then with great probability we can consider it to exist in other forms of life. But the existence of such homogamy is of immense importance for the problem of differentiation. The present statistics do not enable us to say whether homogamy in man is definitely correlated with fertility; they do show that fertility is not a random character, but depends upon the relative size of husband and wife, and thus bring evidence in favour of genetic selection. I can conceive no more valuable investigation than a series of experiments or measurements directed to ascertaining whether homogamy is or is not correlated with fertility, but such investigation, bearing in mind Darwin's conclusions, should carefully distinguish between exogamous and endogamous homogamy.

"On the Numerical Computation of the Functions $G_0(x)$, $G_1(x)$, and $J_n(x\sqrt{i})$." By W. STEADMAN ALDIS, M.A. Communicated by Professor J. J. THOMSON, F.R.S. Received and Read June 15, 1899.

1. The complete solution of the equation

$$\frac{d^2y}{dx^2}+\frac{1}{x}\cdot\frac{dy}{dx}-\left(1+\frac{n^2}{x^2}\right)y = 0$$

may be written

$$y = AI_n(x)+BK_n(x),$$

where

$$I_n(x) = \sum_{r=0}^{r=\infty}\frac{(\frac{1}{2}x)^{n+2r}}{\Pi(r)\,.\,\Pi(n+r)}\quad\ldots\ldots\ldots\ldots(1);$$

and, if n be any positive integer or zero,

$$K_n(x) \doteq EI_n(x) - \Lambda_n(x) \quad \text{.......................(2)},$$

where

$$\Lambda_n(x) = I_n(x)\log x$$
$$+\frac{(-2)^{n-1}\Pi(n-1)}{x^n}\left\{1-\frac{(\frac{1}{2}x)^2}{\overline{n-1}\,.\,1}+\frac{(\frac{1}{2}x)^4}{\overline{n-1}\,.\,\overline{n-2}\,.\,1\,.\,2}+\cdots\cdots\right.$$
$$\left.+\frac{(-1)^{n-1}(\frac{1}{2}x)^{2n-2}}{\{\Pi(n-1)\}^2}\right\}-\tfrac{1}{2}\sum_{r=0}^{r=\infty}\frac{(\frac{1}{2}x)^{n+2r}}{\Pi(r)\,\Pi(n+r)}(S_r+S_{n+r}) \quad \text{......(3)},$$

S_r denoting $1+\frac{1}{2}+\frac{1}{3}+\ldots\ldots+\frac{1}{r}$, with the special case $S_0=0$, and E being $\log 2+\frac{\Gamma'(1)}{\Gamma(1)}$.

2. When x is a real quantity, the function $I_n(x)$ increases from zero (or unity, when $n = 0$) to an infinitely large quantity, as x passes from zero to infinity, while $K_n(x)$ decreases numerically from infinity to zero under the same circumstances.

The values of the functions $K_0(x)$, $K_1(x)$ have been tabulated by the present writer, and published in the 'Proceedings,' for values of x at intervals of 0·1 from 0·1 to 12·0. The elements used in the calculation of the earlier half of these results are available for computing the values of $K_0(x)$ and $K_1(x)$ in some cases when x is a complex quantity.

If x be a pure imaginary $= zi$, z being a scalar, it is easily seen that

$$I_n(x) = i^n J_n(z) \quad \text{............................. (4)},$$

where $J_n(z)$ is the ordinary Bessel's function of the first kind and nth order.

If also $Y_n(z)$ denote Neumann's function of the nth order, and $G_n(z)$ be a function defined by the relation

$$G_n(z) = E\,.\,J_n(z) - Y_n(z) \quad \text{....................... (5)},$$

it can be shown without much difficulty that

$$K_n(x) = i^n G_n(z) - \frac{\pi}{2} i^{n+1} J_n(z) \quad \text{............ ...... (6)}.$$

3. The numerical calculation of the functions $G_0(x)$ and $G_1(x)$ can be made to depend on that of $K_0(x)$ and $K_1(x)$ for any values of x for which the convergent series (1) and (3) are applicable. In doing this it is necessary to calculate the elements of $J_0(x)$ and $J_1(x)$, and incidentally to compute these functions.

With the notation used in the writer's paper on the computation of $K_0(x)$ and $K_1(x)$, it is easily seen that

$$J_0(x) = \beta_0 - \beta_2 + \beta_4 - \beta_6 + \ldots\ldots = \sum_{m=0}^{m=\infty} \beta_{4m} - \sum_{m=0}^{m=\infty} \beta_{4m+2} \ \ldots\ldots (7),$$

$$J_1(x) = \beta_1 - \beta_3 + \beta_5 - \beta_7 + \ldots = \sum_{m=0}^{m=\infty} \beta_{4m+1} - \sum_{m=0}^{m=\infty} \beta_{4m+3} \ \ldots\ldots\ldots (8).$$

Thus the elements β, used in the computation of $I_0(x)$ and $I_1(x)$, for any value of x can be easily used to derive the values of $J_0(x)$ and $J_1(x)$

4. We have further

$$G_0(x) = J_0(x)(E - \log x) - \left\{\frac{(\frac{1}{2}x)^2}{\Pi(1)|^2} - \frac{(\frac{1}{2}x)^4}{\Pi(2)|^2}S_2 - \frac{(\frac{1}{2}x)^6}{\Pi(3)|^2}S_3 - \ldots\ldots\right\}$$

also $$0 = J_0(x) - 1 + \left\{\frac{(\frac{1}{2}x)^2}{\Pi(1)|^2} - \frac{(\frac{1}{2}x)^4}{\Pi(2)|^2} + \frac{(\frac{1}{2}x)^6}{\Pi(3)|^2} - \ldots\ldots\right\}$$

whence by addition

$$G_0(x) = J_0(x)\{E + 1 - \log x\} + \{\gamma_4 - \gamma_6 + \gamma_8 - \ldots\ldots\} - 1 \ \ldots (9),$$

using the notation of the former paper.

Again,

$$G_1(x) = J_1(x)(E - \log x) + \frac{1}{x}$$

$$+ \frac{1}{2}\left\{\frac{x}{2} - \frac{(\frac{1}{2}x)^3(S_1 + S_2)}{\Pi(1)\Pi(2)} + \ldots\ldots + \frac{(-1)^r(\frac{1}{2}x)^{2r+1}(S_r + S_{r+1})}{\Pi(r)\Pi(r+1)} + \ldots\right\}$$

but $$0 = J_1(x) - \left\{\frac{x}{2} - \frac{(\frac{1}{2}x)^3}{\Pi(1)\Pi(2)} + \ldots\ldots + \frac{(-1)^r(\frac{1}{2}x)^{2r+1}}{\Pi(r).\Pi(r+1)} + \ldots\ldots\right\};$$

whence, adding,

$$G_1(x) = J_1(x)\{E + 1 - \log x\} + \frac{1}{x} - \frac{x}{4}$$

$$- \frac{(\frac{1}{2}x)^3(S_1 + S_2 - 2)}{2\Pi(1)\Pi(2)} + \ldots\ldots + \frac{(-1)^r(\frac{1}{2}x)^{2r+1}(S_r + S_{r+1} - 2)}{2\Pi(r)\Pi(r+1)} \ldots\ldots$$

But

$$\frac{(\frac{1}{2}x)^3(S_1 + S_2 - 2)}{2\Pi(1)\Pi(2)} = \frac{(\frac{1}{2}x)^3(S_1 - 1)}{\Pi(1)\Pi(2)} + \frac{1}{8}\left(\frac{x}{2}\right)^3 = \frac{\beta_4}{x}$$

....

$$\frac{(\frac{1}{2}x)^{2r+1}(S_r + S_{r+1} - 2)}{2\Pi(r)\Pi(r+1)} = \frac{(\frac{1}{2}x)^{2r+1}(S_r - 1)}{\Pi(r)\Pi(r+1)} + \frac{(\frac{1}{2}x)^{2r+1}}{2\{\Pi(r+1)\}^2}$$

$$= \frac{\frac{x}{2}\gamma_{2r}}{r+1} + \frac{\beta_{2r+2}}{x}$$

Hence

$$G_1(x) = J_1(x)\{E+1-\log x\} + \frac{1}{x} - \frac{x}{4}$$
$$+\frac{x}{2}\left\{\frac{1}{3}\gamma_4 - \frac{1}{4}\gamma_6 + \frac{1}{5}\gamma_8 - \frac{1}{6}\gamma_{10} + \ldots\right\} - \frac{1}{x}\{\beta_4 - \beta_6 + \beta_8 - \ldots\}.$$

The last portion of this $= -\frac{1}{x}\{J_0(x) - 1 + \beta_2\} = -\frac{J_0(x)}{x} + \frac{1}{x} - \frac{x}{4}$,

whence $$G_1(x) = J_1(x)\{E+1-\log x\} + \frac{2}{x} - \frac{x}{2}$$
$$+\frac{x}{2}\left\{\frac{1}{3}\gamma_4 - \frac{1}{4}\gamma_6 + \frac{1}{5}\gamma_8 - \frac{1}{6}\gamma_{10} + \ldots\right\} - \frac{J_0(x)}{x} \ldots\ldots (10)$$

5. The quantities β and γ, $\frac{\gamma_{2r}}{r+1}$, and the multiples of the different values of $(E+1-\log x)$ have been computed for the values of x, 0·1, 0·2,......, 6·0, in the process of calculation of $K_0(x)$ and $K_1(x)$, given in the writer's former paper. It has been, therefore, an easy matter to find by (7), (8), (9), and (10), the quantities $J_0(x)$, $J_1(x)$, $G_0(x)$, and $G_1(x)$ corresponding to the same values of x. The former two are of course well known, but the recalculation affords a valuable verification of the correctness of the quantities β. The results are given in Table I, appended to this paper, negative values being indicated by the use of old numeral type.

The formula used for verifying the values of I and K was

$$I_1(x) \cdot K_0(x) - I_0(x) \cdot K_1(x) = \frac{1}{x}.$$

Replacing x by zi, by means of (4) and (6), this gives

$$iJ_1(z)\left\{G_0(z) - \frac{\pi}{2}iJ_0(z)\right\} - J_0(z)\left\{iG_1(z) + \frac{\pi}{2}J_1(z)\right\} = \frac{1}{zi}$$

whence $$J_1(z) \cdot G_0(z) - J_0(z) \cdot G_1(z) = -\frac{1}{z} \ldots\ldots\ldots\ldots\ldots\ldots\ldots (11).$$

This formula has been applied throughout Table I to each set of four values, calculated to three places beyond those given. Where the last figure has been increased by unity, in consequence of the first omitted figure being equal to or greater than five, the fact is indicated by a dot after the last figure. The column $G_0(x)$ has also been tested with satisfactory results by differencing.

6. The value of $I_n(x)$ can be readily expressed in terms of the quantities β, when n is either zero or unity, in one or two other cases, beside those of x, being a pure imaginary or wholly real.

For instance if $$x = z\epsilon^{\frac{1}{4}(i\pi)} = zi^{\frac{1}{2}},$$

then $$I_0(zi^{\frac{1}{2}}) = P_0 + Q_0 i, \text{ say},$$

where $$P_0 = \beta_0 - \beta_4 + \beta_8 - \ldots\ldots \qquad Q_0 = \beta_2 - \beta_6 + \beta_{10} - \ldots\ldots$$

Thus the values of P_0 and Q_0 are easily deduced, and, therefore, that of $I_0(xi^{\frac{1}{2}})$.

The same process gives the value of $J_0(xi^{\frac{1}{2}})$, for,

since $$J_0(x) = \beta_0 - \beta_2 + \beta_4 - \beta_6 + \ldots\ldots$$

it is easily seen that

$$J_0(xi^{\frac{1}{2}}) = \beta_0 + \beta_2 i - \beta_4 + \beta_6 \dot{2} + \beta_8 - \ldots\ldots = P_0 - Q_0 i \ldots\ldots\ldots(12).$$

The values of P_0 and Q_0 are tabulated in the Report of the British Association for 1893, to nine places of decimals for intervals of 0·2 of a unit. Table II at the end of this paper gives them for the same number of places, and for the same intervals as have been used in the calculation of the K and G functions.

P_0 and Q_0 are denoted in the Table II by X and Y in accordance with the notation adopted by the Committee of the British Association, negative values being denoted by the use of old numeral type.

7. Assuming the accuracy of the values used for the quantities β, an accuracy guaranteed by the tests to which the Tables for I and K in the former paper have been subjected, the relation between I and J gives a very easy check for detecting and correcting any mistakes in addition or copying figures in finding the values of J.

Thus

$$I_0(x) = \sum_{m=0}^{m=\infty} \beta_{4m} + \Sigma\beta_{4m+2}$$

$$J_0(x) = \Sigma\beta_{4m} - \Sigma\beta_{4m+2}.$$

In finding $J_0(x)$, $\Sigma\beta_{4m}$ and $\Sigma\beta_{4m+2}$ are separately computed by addition of alternate terms from $I_0(x)$, and the smaller sum written down below the larger. In all cases in Table I the sum of these has first been taken, and the agreement or disagreement of this sum with the known correct value of $I_0(x)$ has shown either that there was no mistake, or has revealed where such mistake was committed, and secured its correction.

A similar test of accuracy in finding $J_1(x)$ is derived from the known values of $I_1(x)$.

In like manner, since

$$\Sigma\beta_{4m} = \Sigma\beta_{8m} + \Sigma\beta_{8m+4},$$

and $$X = P_0 = \Sigma\beta_{8m} - \Sigma\beta_{8m+4},$$

the known value of $\Sigma\beta_{4m}$, obtained in finding $J_0(x)$, gives a check on mistakes in calculating X. The known value of $\Sigma\beta_{4m+2}$ does the same service in regard to the computation of Q_0 or Y.

8. By formula (8)

$$J_1(x) = \beta_1 - \beta_3 + \beta_5 - \beta_7 + \ldots\ldots .$$

Hence $$J_1(x\sqrt{i}) = \beta_1 i^{\frac{1}{2}} - \beta_3 i^{\frac{3}{2}} + \beta_5 i^{\frac{5}{2}} - \ldots\ldots$$

$$= \beta_1 \cos\frac{\pi}{4} - \beta_3 \cos\frac{3\pi}{4} + \beta_5 \cos\frac{5\pi}{4} - \ldots\ldots$$

$$+ i\left(\beta_1 \sin\frac{\pi}{4} - \beta_3 \sin\frac{3\pi}{4} + \beta_5 \sin\frac{5\pi}{4} \ldots\ldots\right)$$

$$= \frac{1}{\sqrt{2}}\{\overline{\beta_1 + \beta_3} - \overline{\beta_5 - \beta_7} + \overline{\beta_9 + \beta_{11}} - \overline{\beta_{13} - \beta_{15}} + \ldots\ldots$$

$$+ i(\overline{\beta_1 - \beta_3} - \overline{\beta_5 + \beta_7} + \overline{\beta_9 - \beta_{11}} - \overline{\beta_{13} + \beta_{15}} + \ldots\ldots)\}$$

$$= \frac{1}{\sqrt{2}}\{X_1 + Y_1 i\}\ldots\ldots, \text{say} \quad \ldots\ldots \quad (13),$$

where

$$X_1 = \Sigma(\beta_{8m+1} + \beta_{8m+3}) - \Sigma(\beta_{8m+5} + \beta_{8m+7}),$$

$$Y_1 = \Sigma(\beta_{8m+1} + \beta_{8m+7}) - \Sigma(\beta_{8m+3} + \beta_{8m+5}),$$

the summation being in all cases from $m = 0$ to the largest value of m which gives sensible values for β.

The values of X_1, Y_1, computed by these formulæ from the known values of β, are given in Table II.

The computations evidently admit of a check to inaccuracy of the same nature as those given in the last article.

Another form of the values of X_1 and Y_1 is given by

$$X_1 = \Sigma(\beta_{8m+1} - \beta_{8m+5}) + \Sigma(\beta_{8m+3} - \beta_{8m+7}),$$

$$Y_1 = \Sigma(\beta_{8m+1} - \beta_{8m+5}) - \Sigma(\beta_{8m+3} - \beta_{8m+7}),$$

which reduces the computation to that of the two quantities

$$\Sigma(\beta_{8m+1} - \beta_{8m+5}) \text{ and } \Sigma(\beta_{8m+3} - \beta_{8m+7}),$$

so that if these be denoted by P_1 and Q_1

$$X_1 = P_1 + Q_1, \qquad Y_1 = P_1 - Q_1.$$

This form admits of somewhat different checks to mistakes. The values in Table II have been computed independently in the two ways, so that the writer has every confidence that they may be relied on as correct. The column for Y_1 has also been differenced with satisfactory *results.*

9. The well-known sequence laws

$$J_{n+1}(x) = \frac{2n}{x} J_n(x) - J_{n-1}(x) \quad \text{.................} \quad (14),$$

$$dJ_0/dx = -J_1 \quad \text{..............................} \quad (15)$$

can be utilised, the former to obtain the values of $J_2(xi^{\frac{1}{2}})$, $J_3(xi^{\frac{1}{2}})$ ..., and the latter to give a verification to some extent of the values of $J_1(xi^{\frac{1}{2}})$, by means of the formulæ given in the writer's paper on I and K, which express dy/dx in terms of a series of equidistant values of y.

Thus, since

$$dJ_0/dx = -J_1,$$

replacing x by $xi^{\frac{1}{2}}$, and using the values already assumed for $J_0(xi^{\frac{1}{2}})$ and $J_1(xi^{\frac{1}{2}})$, it follows that

$$\frac{d(X - Yi)}{dx} = -i^{\frac{1}{2}} \cdot \frac{X_1 + Y_1 i}{\sqrt{2}} = -\frac{1+i}{\sqrt{2}} \cdot \frac{X_1 + Y_1 i}{\sqrt{2}}.$$

Whence

$$\left.\begin{aligned} dX/dx &= -\tfrac{1}{2}(X_1 - Y_1), \\ dY/dx &= \tfrac{1}{2}(X_1 + Y_1) \end{aligned}\right\} \quad \text{.................} \quad (16).$$

By means of the formulæ (18), (19), and (21) of Articles 17—19 in the paper above referred to, this formula gives a check to the series of values in Table II to a considerable number of decimal places, to thirteen places with the last approximation.

10. For determining the values of $J_2(xi^{\frac{1}{2}})$, $J_3(xi^{\frac{1}{2}})$, ... by the sequence law, it is convenient to denote these quantities by the symbol $X_n + Y_n i$ when n is even, and by $\frac{1}{\sqrt{2}}(X_n + Y_n i)$ when n is odd. This will be found to avoid irrational multipliers in the successive derivations.

Equation (14), putting $xi^{\frac{1}{2}}$ for x, gives

$$J_{n+1}(xi^{\frac{1}{2}}) = \frac{2n}{xi^{\frac{1}{2}}} \cdot J_n(xi^{\frac{1}{2}}) - J_{n-1}(xi^{\frac{1}{2}}).$$

The cases of n odd and n even must be separately considered.

First let n be odd. The equation gives, remembering that $i^{-\frac{1}{2}} = \frac{1-i}{\sqrt{2}}$,

$$X_{n+1} + Y_{n+1} i = \frac{n}{x}(1 - i)(X_n + Y_n i) - (X_{n-1} + Y_{n-1} i).$$

Whence, if n be odd,

$$\left.\begin{aligned} X_{n+1} &= \frac{n}{x}(X_n + Y_n) - X_{n-1} \\ Y_{n+1} &= \frac{n}{x}(Y_n - X_n) - Y_{n-1} \end{aligned}\right\} \quad \text{..............} \quad (17).$$

If, secondly, n be even, the equation gives

$$\frac{1}{\sqrt{2}}(X_{n+1}+iY_{n+1}) = \frac{2n}{x}\frac{1-i}{\sqrt{2}}(X_n+Y_n i) - \frac{1}{\sqrt{2}}(X_{n-1}+Y_{n-1}i),$$

whence

$$\left.\begin{aligned} X_{n+1} &= \frac{2n}{x}(X_n+Y_n) - X_{n-1} \\ Y_{n+1} &= \frac{2n}{x}(Y_n-X_n) - Y_{n-1} \end{aligned}\right\} \qquad (18).$$

The most important special cases are when $n = 1$ and $n = 2$. In the first, remembering that $X_0 = X$, $Y_0 = -Y$ (Article 6), equations (17) give

$$\left.\begin{aligned} X_2 &= \frac{X_1+Y_1}{x} - X \\ Y_2 &= \frac{Y_1-X_1}{x} + Y \end{aligned}\right\} \qquad (19).$$

In the second case (18) gives

$$\left.\begin{aligned} X_3 &= \frac{4}{x}(X_2+Y_2) - X_1 \\ Y_3 &= \frac{4}{x}(Y_2-X_2) - Y_1 \end{aligned}\right\} \qquad (20).$$

In these derivations no labour is involved, except that of addition of known quantities, and division by x.

Table I.

x.	$J_0(x)$.	$J_1(x)$.
0·1	0·997 501 562 066 040 082 281	0·049 937 526 036 241 997 556
0·2	0·990 024 972 239 576 390 818·	0·099 500 832 639 235 995 398·
0·3	0·977 626 246 538 296 087 570·	0·148 318 816 273 104 007 741
0·4	0·960 398 226 659 563 450 344	0·196 026 577 955 318 744 107·
0·5	0·938 469 807 240 812 904 228	0·242 268 457 674 873 886 384·
0·6	0·912 004 863 497 210 775 955·	0·286 700 988 063 915 739 746·
0·7	0·881 200 888 607 405 280 839·	0·328 995 741 540 058 947 849·
0·8	0·846 287 352 750 480 266 089·	0·368 842 046 094 169 994 205
0·9	0·807 523 798 122 544 777 302	0·405 949 546 078 805 674 605·
1·0	0·765 197 686 557 966 551 450·	0·440 050 585 744 933 515 960·
1·1	0·719 622 018 527 511 015 975·	0·470 902 394 866 292 936 849·
1·2	0·671 132 744 264 362 673 475	0·498 289 057 567 215 480 211·
1·3	0·620 085 989 561 509 131 673	0·522 023 247 414 660 396 129·
1·4	0·566 855 120 374 288 721 361·	0·541 947 713 930 854 583 158·
1·5	0·511 827 671 735 918 128 749	0·557 936 507 910 099 641 990
1·6	0·455 402 167 639 380 713 311	0·569 895 935 261 680 370 018
1·7	0·397 984 859 446 109 491 142·	0·577 765 231 529 023 219 798
1·8	0·339 986 411 042 558 350 093	0·581 516 951 731 165 183 470
1·9	0·281 818 559 374 385 470 714·	0·581 157 072 713 434 072 686·
2·0	0·223 890 779 141 235 668 052·	0·576 724 807 756 878 387 202
2·1	0·166 606 980 331 990 326 602	0·568 292 135 757 038 668 540·
2·2	0·110 362 266 922 173 950 988·	0·555 963 049 819 063 939 102
2·3	0·055 539 784 445 601 963 144	0·539 872 532 604 313 665 317
2·4	0·002 507 683 297 243 813 015	0·520 185 268 181 931 033 964
2·5	0·048 383 776 468 197 996 327	0·497 094 102 464 274 038 011·
2·6	0·096 804 954 397 038 249 909	0·470 818 266 517 578 669 738·
2·7	0·142 449 370 046 011 821 820	0·441 601 379 118 253 106 422
2·8	0·185 036 033 364 387 324 596	0·409 709 246 852 288 741 579
2·9	0·224 311 545 791 968 114 187·	0·375 427 481 813 095 896 391·
3·0	0·260 051 954 901 933 437 624	0·339 058 958 525 936 458 926·
3·1	0·292 064 347 650 697 540 058	0·300 921 133 101 057 626 662
3·2	0·320 188 169 657 122 907 289	0·261 343 248 780 504 837 363·
3·3	0·344 296 260 398 884 637 389	0·220 663 452 985 241 082 698
3·4	0·364 295 596 762 000 469 831·	0·179 225 851 681 507 110 994
3·5	0·380 127 739 987 263 377 379·	0·137 377 527 362 327 185 716
3·6	0·391 768 983 700 797 768 519·	0·095 465 547 177 876 403 846·
3·7	0·399 230 203 371 191 105 766	0·053 833 987 745 461 864 015
3·8	0·402 556 410 178 564 169 319	0·012 821 002 926 731 627 029
3·9	0·401 826 014 887 639 905 035·	0·027 244 039 620 779 926 253
4·0	0·397 149 809 863 847 372 287·	0·066 043 328 023 549 136 143
4·1	0·388 669 679 835 853 683 029	0·103 273 257 747 338 701 790·
4·2	0·376 557 054 367 567 663 516·	0·138 646 942 126 046 167 310·
4·3	0·361 011 117 236 535 112 103	0·171 896 560 221 540 474 678·
4·4	0·342 256 790 003 885 614 439·	0·202 775 521 923 086 594 695
4·5	0·320 542 508 985 121 424 355	0·231 060 431 923 370 634 008
4·6	0·296 137 816 574 141 142 650·	0·256 552 836 097 444 561 708·
4·7	0·269 330 789 419 752 826 396	0·279 080 735 843 335 330 140
4·8	0·240 425 327 291 183 452 194·	0·298 499 858 099 557 876 149·
4·9	0·209 738 327 585 326 314 755·	0·314 694 671 015 190 603 203·
5·0	0·177 596 771 314 338 304 347	0·327 579 137 591 465 222 038·
5·1	0·144 334 747 060 500 516 529	0·337 097 202 018 231 840 465·
5·2	0·110 290 439 790 986 539 621	0·343 223 005 871 921 903 218·
5·3	0·075 803 111 585 584 160 063·	0·345 960 833 801 186 199 542
5·4	0·041 210 101 244 991 307 084·	0·345 344 790 779 586 326 575
5·5	0·006 843 869 417 819 196 824·	0·341 438 215 429 043 350 180·
5·6	0·026 970 884 685 114 476 356·	0·334 332 836 291 007 483 208
5·7	0·059 920 009 724 037 401 926·	0·324 147 680 222 856 214 217
5·8	0·091 702 567 574 816 188 248	0·311 027 744 303 942 414 148
5·9	0·122 033 354 592 822 673 484·	0·295 142 444 729 016 123 857·
6·0	0·150 645 257 250 996 931 662	0·276 683 858 127 565 608 173·

N.B.—Negative quantities are

Table I.

$G_0(x)$.	$G_1(x)$.	x.
2·409 976 437 967 912 294 552	10·145 696 654 505 820 445 994	0·1
1·698 196 269 260 531 005 616·	5·221 052 082 235 180 455 883·	0·2
1·268 062 370 733 913 360 785	3·602 001 128 335 204 510 007	0·3
0·951 941 166 032 609 089 045·	2·797 387 265 631 115 266 589	0·4
0·698 248 393 783 854 194 778	2·311 383 429 386 515 572 834	0·5
0·484 606 170 757 539 963 706·	1·979 818 098 470 311 722 022	0·6
0·299 405 770 651 788 694 072·	1·732 980 846 329 450 701 757·	0·7
0·136 348 702 042 021 281 732·	1·536 465 279 810 038 555 299	0·8
0·008 840 923 388 656 204 883·	1·371 504 028 549 382 729 782	0·9
0·138 633 715 204 053 999 681	1·227 126 230 143 571 489 243	1·0
0·254 725 363 498 849 106 693·	1·096 603 640 617 960 561 767	1·1
0·358 272 729 071 792 761 119	0·975 678 743 748 170 263 436·	1·2
0·450 088 686 532 541 263 114	0·861 612 777 028 331 588 570·	1·3
0·530 764 428 542 739 360 172·	0·752 642 307 119 771 213 489·	1·4
0·600 749 364 688 180 915 674	0·647 652 876 756 467 095 194	1·5
0·660 405 024 575 635 605 540	0·545 974 258 657 556 467 494	1·6
0·710 042 351 497 427 739 063·	0·447 246 939 888 742 173 248·	1·7
0·749 947 984 061 056 004 754·	0·351 331 953 359 999 750 701	1·8
0·780 402 985 970 970 798 205·	0·258 247 983 282 347 884 849·	1·9
0·801 696 231 883 694 215 426·	0·168 126 150 312 430 935 228·	2·0
0·814 133 899 087 413 666 664	0·081 176 574 108 327 549 604	2·1
0·818 046 042 540 011 194 399·	0·002 337 013 951 404 941 779	2·2
0·813 790 929 365 743 495 271·	0·082 117 015 702 804 981 444·	2·3
0·801 757 612 346 090 680 037	0·157 847 655 213 986 366 030	2·4
0·782 367 091 369 019 468 035·	0·229 207 675 130 978 077 462	2·5
0·756 072 323 668 009 246 676	0·295 880 763 567 315 512 986·	2·6
0·723 357 283 363 643 701 757	0·357 564 209 833 291 131 344	2·7
0·684 735 229 033 948 656 974	0·413 976 136 265 745 314 572·	2·8
0·640 746 308 772 709 085 294	0·464 861 550 729 216 627 288	2·9
0·591 954 611 480 711 143 919·	0·509 997 393 867 205 323 674·	3·0
0·538 944 758 310 761 413 627	0·549 196 706 485 298 624 293	3·1
0·482 318 117 438 413 367 641·	0·582 312 008 724 774 045 611·	3·2
0·422 688 717 401 572 140 599	0·609 237 959 388 677 935 217	3·3
0·360 678 928 264 659 951 052	0·629 913 347 832 572 686 549·	3·4
0·296 914 975 194 465 215 873·	0·644 322 460 111 513 733 523	3·5
0·232 022 345 240 471 206 452·	0·652 495 854 105 516 078 102·	3·6
0·166 621 144 872 495 980 327	0·654 510 574 081 292 622 136	3·7
0·101 321 462 912 008 607 214·	0·650 489 832 836 477 094 620	3·8
0·036 718 790 734 042 399 784	0·640 602 188 665 708 742 081	3·9
0·026 610 451 105 001 945 410	0·625 060 244 480 341 903 495·	4·0
0·088 113 233 426 177 404 311	0·604 118 897 200 782 671 826·	4·1
0·147 264 042 657 322 775 155	0·578 073 166 790 814 300 183·	4·2
0·203 568 768 228 724 991 685	0·547 255 635 837 897 775 759·	4·3
0·256 568 315 804 579 439 611·	0·512 033 532 262 535 404 617·	4·4
0·305 841 912 363 794 369 172·	0·472 805 489 452 910 279 056·	4·5
0·351 010 072 657 030 554 463	0·429 998 019 780 139 344 891·	4·6
0·391 737 200 240 423 447 540·	0·384 061 738 984 071 875 442	4·7
0·427 733 800 104 815 454 258	0·335 467 380 273 038 857 255	4·8
0·458 758 283 862 071 400 448·	0·284 701 638 108 088 275 409	4·9
0·484 618 352 492 666 714 073	0·232 262 882 507 286 221 237·	5·0
0·505 171 945 773 571 858 198·	0·178 656 785 280 977 272 574	5·1
0·520 327 751 662 450 130 166	0·124 391 899 873 886 134 313	5·2
0·530 045 273 081 540 584 629·	0·069 975 236 430 273 453 649	5·3
0·534 334 453 689 368 500 921	0·015 907 873 305 342 552 148·	5·4
0·533 254 868 317 059 586 380·	0·037 319 354 483 812 230 517·	5·5
0·526 914 487 744 863 636 272	0·089 230 050 440 030 414 040·	5·6
0·515 468 031 370 191 891 880·	0·139 366 297 368 738 402 285	5·7
0. 499 114 925 038 474 697 901·	0·187 292 507 445 744 860 142	5·8
0·478 096 884 841 038 443 599	0·232 599 047 277 909 357 025	5·9
0·452 695 151 000 080 566 867	0·274 905 605 978 175 743 452·	6·0

denoted by old numeral type.

Table II.

$J_0(x\sqrt{1}) = X - Yi$

x.	X	Y
0·1	0·999 998 437 500 067 816 840·	0·002 499 999 565 972 229 004·
0·2	0·999 975 000 017 361 109 182	0·009 999 972 222 229 166 666
0·3	0·999 873 437 944 946 038 780	0·022 499 683 594 150 451 545
0·4	0·999 600 004 444 436 543 214·	0·039 998 222 229 383 326 883
0·5	0·999 023 463 990 838 255 555·	0·062 493 218 382 199 458 650
0·6	0·997 975 113 905 224 846 398·	0·089 979 750 410 060 617 063·
0·7	0·996 248 828 444 070 123 287	0·122 448 988 981 613 810 260
0·8	0·993 601 137 745 414 585 178	0·159 886 229 503 894 323 928
0·9	0·989 751 356 659 594 009 089	0·202 269 363 489 470 399 618
1·0	0·984 381 781 213 086 883 966·	0·249 566 040 036 659 721 419
1·1	0·977 137 973 163 994 306 095·	0·301 731 269 206 265 863 908·
1·2	0·967 629 155 801 133 528 979·	0·358 704 419 873 150 681 448
1·3	0·955 428 746 808 400 572 511	0·420 405 965 634 100 168 746
1·4	0·940 075 056 652 724 712 846	0·486 733 933 588 908 060 448·
1·5	0·921 072 183 546 255 764 122	0·557 560 062 303 086 694 894
1·6	0·897 891 138 567 705 276 346	0·632 725 677 031 398 154 882·
1·7	0·869 971 236 987 757 520 821·	0·712 037 292 354 219 242 730·
1·8	0·836 721 794 210 160 854 515·	0·795 261 954 775 658 372 738·
1·9	0·797 524 166 991 521 789 701	0·882 122 340 574 509 297 086
2·0	0·751 734 182 713 808 228 551·	0·972 291 627 306 661 206 104
2·1	0·698 685 001 425 635 398 101·	1·065 388 160 849 286 232 192·
2·2	0·637 690 457 109 552 833 002	1·160 969 943 770 221 785 831·
2·3	0·568 048 926 137 096 187 234	1·258 528 975 115 816 306 982·
2·4	0·489 047 772 101 826 069 086·	1·357 485 476 450 273 287 287
2·5	0·399 968 417 129 531 339 957	1·457 182 044 159 804 184 047·
2·6	0·300 092 090 306 787 850 787·	1·556 877 773 663 311 509 857
2·7	0·188 706 303 992 608 423 524	1·655 742 407 252 085 252 722
2·8	0·065 112 108 427 346 531 305	1·752 850 563 814 438 038 253
2·9	0·071 367 825 831 445 002 541·	1·847 176 115 683 253 092 922·
3·0	0·221 380 249 598 693 888 868	1·937 586 785 266 042 766 897·
3·1	0·385 531 454 977 281 413 314·	2·022 839 041 963 733 753 825
3·2	0·564 376 430 484 566 549 458·	2·101 573 388 135 250 371 321
3·3	0·758 407 012 072 785 084 982	2·172 310 131 492 460 325 998·
3·4	0·968 038 995 314 976 506 884·	2·233 445 750 279 040 972 132·
3·5	1·193 598 179 589 928 060 082·	2·283 249 966 853 914 618 212
3·6	1·435 305 321 718 847 744 816·	2·319 863 654 812 663 506 793·
3·7	1·693 259 984 269 599 885 400	2·341 297 714 476 542 058 301·
3·8	1·967 423 272 739 419 648 007	2·345 433 061 385 529 680 393·
3·9	2·257 599 466 142 987 708 599	2·330 021 882 265 074 524 014·
4·0	2·563 416 557 258 579 754 134·	2·292 690 322 699 299 833 586·
4·1	2·884 305 732 008 850 753 468·	2·230 942 780 326 965 102 027
4·2	3·219 479 832 260 939 763 946·	2·142 167 986 657 022 889 923·
4·3	3·567 910 862 806 221 604 427·	2·023 647 069 440 171 807 909
4·4	3·928 306 621 502 089 386 988·	1·872 563 795 777 954 293 134
4·5	4·299 086 551 599 756 238 427	1·686 017 203 632 139 319 953·
4·6	4·678 356 937 208 980 936 827·	1·461 036 835 928 036 069 728
4·7	5·063 885 586 719 503 887 521·	1·194 600 796 822 301 663 253·
4·8	5·453 076 174 855 458 180 119	0·883 656 853 707 154 174 111
4·9	5·842 942 441 915 628 551 218	0·525 146 810 908 826 889 589·
5·0	6·230 082 478 666 357 733 185	0·116 034 381 550 200 378 097
5·1	6·610 653 357 304 570 918 646	0·346 663 217 591 247 641 801
5·2	6·980 346 402 874 876 505 440·	0·865 839 727 484 430 267 303·
5·3	7·334 363 435 462 957 925 254·	1·444 260 150 604 921 519 731
5·4	7·667 394 351 327 397 532 141	2·084 516 693 093 664 203 000
5·5	7·973 596 450 774 417 438 658·	2·788 980 154 734 066 597 920·
5·6	8·246 575 961 893 122 136 086	3·559 746 593 355 732 201 313·
5·7	8·479 372 252 085 205 623 568	4·398 579 111 649 335 813 378·
5·8	8·664 445 263 435 904 450 574	5·306 844 640 335 221 439 301·
5·9	8·793 666 753 132 378 304 231	6·285 445 622 573 310 185 248
6·0	8·858 315 966 045 036 088 551	7·334 746 540 847 962 419 331·

N.B.—Negative quantities are

Table II.

$$J_1(x\sqrt{i}) = \frac{1}{\sqrt{2}}(X_1 + Y_1 i)$$

X_1	Y_1	
0·050 062 473 952 908 664 336	0·049 937 473 963 759 358 667	0·1
0·100 499 165 972 569 560 158·	0·099 499 167 361 458 217 565	0·2
0·151 681 160 023 124 019 598	0·148 306 183 753 572 747 019	0·3
0·203 973 244 622 459 033 349·	0·195 973 422 399 762 737 375·	0·4
0·257 730 697 263 717 955 586	0·242 106 544 968 702 488 384	0·5
0·313 295 988 104 834 119 210	0·286 299 025 563 828 010 431·	0·6
0·370 995 377 179 567 178 556·	0·328 129 312 969 530 045 753	0·7
0·431 135 380 394 708 350 508·	0·367 158 134 979 370 769 977	0·8
0·493 999 080 858 644 866 754	0·402 925 975 846 922 443 874·	0·9
0·559 842 263 647 128 287 093·	0·434 950 759 289 066 366 621	1·0
0·628 889 353 965 588 933 103·	0·462 725 771 056 724 536 018·	1·1
0·701 329 140 828 180 143 887·	0·485 717 856 852 007 564 806	1·2
0·777 310 270 858 051 149 439·	0·503 365 933 293 808 160 710	1·3
0·856 936 499 638 710 654 481·	0·515 079 851 693 132 327 170	1·4
0·940 261 691 231 561 790 777	0·520 239 656 568 985 033 820·	1·5
1·027 284 560 039 561 065 790	0·518 195 283 086 146 165 860	1·6
1·117 943 153 161 996 231 566·	0·508 266 739 894 923 728 550	1·7
1·212 109 075 771 521 480 011	0·489 744 826 163 507 446 244	1·8
1·309 581 466 872 838 661 198·	0·461 892 433 875 541 892 607·	1·9
1·410 080 738 093 475 393 276	0·423 946 488 674 597 149 781	2·0
1·513 242 093 930 976 259 500	0·375 120 584 624 677 257 030	2·1
1·618 608 858 156 316 276 466	0.314 608 370 168 666 686 818·	2·2
1·725 625 637 867 629 817 384	0·241 587 744 246 948 343 429	2·3
1·833 631 364 016 387 962 836	0·155 225 922 923 833 261 156	2·4
1·941 852 255 102 137 149 639·	0·054 685 437 892 511 240 814	2·5
2·049 394 759 160 608 253 323·	0·060 868 871 182 468 533 464	2·6
2·155 238 538 158 111 742 033	0·192 261 808 450 176 681 909	2·7
2·258 229 568 452 541 032 652	0·340 298 656 269 695 509 185	2·8
2·357 073 441 082 328 838 113·	0·505 754 831 903 598 931 394·	2·9
2·450 328 956 287 265 629 836	0·689 364 308 171 542 723 711	3·0
2·536 402 117 829 866 100 448·	0·891 806 742 577 774 109 948·	3·1
2·613 540 644 345 464 751 601	1·113 693 263 253 173 849 031·	3·2
2·679 829 127 066 822 862 186·	1·355 550 864 962 340 196 186·	3·3
2·733 184 975 798 009 493 162	1·617 805 374 530 433 355 956·	3·4
2·771 355 307 894 804 733 452	1·900 762 952 457 942 309 529	3·5
2·791 914 948 174 775 185 144	2·204 590 106 332 283 224 110·	3·6
2·792 265 721 046 090 235 560·	2·529 292 202 033 743 119 928·	3·7
2·769 637 229 612 254 898 369·	2·374 690 470 791 989 051 438·	3·8
2·721 089 329 966 841 647 382·	3·240 397 524 001 181 798 218	3·9
2·643 516 522 207 920 785 664	3·625 791 403 469 366 015 499	4·0
2·533 654 492 728 293 864 014·	4·029 988 212 582 420 082 983	4·1
2·388 089 054 907 953 209 681	4·451 813 393 822 667 265 913·	4·2
2·203 267 747 262 508 230 792·	4·889 771 740 310 993 322 800·	4·3
1·975 514 359 177 636 802 071	5·342 016 253 646 428 366 716	4·4
1·701 046 664 354 870 983 541	5·806 315 987 397 841 481 769·	4·5
1·375 997 650 755 172 598 883·	6·280 023 045 247 471 131 240·	4·6
0·996 440 542 876 983 259 127·	6·760 038 935 071 492 457 005·	4·7
0·558 417 917 343 477 630 173·	7·242 780 515 229 332 115 515·	4·8
0·057 975 215 673 352 365 710·	7·724 145 807 063 384 231 105	4·9
0·508 801 041 580 956 398 748·	8·199 479 988 105 265 446 223	5·0
1·145 740 037 801 321 482 227·	8·663 541 923 740 282 796 986	5·1
1·856 538 144 879 196 341 558	9·110 471 641 066 744 879 434·	5·2
2·644 704 823 723 608 609 532·	9·533 759 197 337 575 219 035	5·3
3·513 502 669 101 221 083 206	9·926 215 446 588 907 115 423·	5·4
4·465 881 346 006 416 880 626·	10·279 945 261 704 303 219 887	5·5
5·504 405 188 780 255 648 900	10·586 323 825 049 679 446 625·	5·6
6·631 174 263 958 089 750 823·	10·835 976 658 709 369 731 829·	5·7
7·847 738 733 862 387 803 316·	11·018 764 124 970 370 273 237·	5·8
9·155 006 400 820 864 395 768	11·123 771 188 692 843 678 508·	5·9
10·553 143 362 143 207 874 207·	11·139 303 295 159 365 990 786·	6·0

denoted by old numeral type.

December 14, 1899.

Dr. G. J. STONEY, Vice-President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Right Hon. Lord Justice Romer, a member of Her Majesty's Most Honourable Privy Council, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

I. "The Piscian Stars." By Sir NORMAN LOCKYER, F.R.S.

II. "On the Origin of certain Unknown Lines in the Spectra of Stars of the β Crucis Type, and on the Spectrum of Silicon." By JOSEPH LUNT. Communicated by Dr. GILL, F.R.S.

III. "A Note on the Electrical Resistivity of Electrolytic Nickel." By Professor J. A. FLEMING, F.R.S.

IV. "Investigations on Platinum Thermometry at Kew Observatory." By Dr. C. CHREE, F.R.S.

V. "Observations on the Morphology of the Blastomycetes found in Carcinomata." By Dr. K. W. MONSARRAT. Communicated by Professor SHERRINGTON, F.R.S.

The Society adjourned over the Christmas Recess to Thursday, January 18, 1900.

"On the Origin of certain Unknown Lines in the Spectra of Stars of the β Crucis Type and on the Spectrum of Silicon." By JOSEPH LUNT, B.Sc., F.I.C., Assistant, Royal Observatory Cape of Good Hope. Communicated by DAVID GILL, C.B., F.R.S., Her Majesty's Astronomer at the Cape. Received November 27—Read, December 14, 1899.

In a recent paper "On the presence of Oxygen in the Atmospheres of certain Fixed Stars,"* Dr. Gill calls attention to three unknown lines

* 'Roy. Soc. Proc.,' vol. 65, p. 205.

in the spectra of β Crucis, ϵ Canis Majoris, and stars of their type, viz., wave-lengths 4552·79, 4567·09, 4574·68.

Mr. McClean had previously also recorded these lines in his measures of the spectrum of β Crucis* as wave lengths 4552·6, 4567·5, 4574·5, but beyond pointing out the approximate coincidence of the first of these with lines due to barium or titanium, he assigns no origin to them.

Sir Norman Lockyer frequently records them as unknown lines. In his recent paper "On the appearance of the Cleveite and other New Gas Lines in the Hottest Stars" (June, 1897),† he records all three lines as unknown. The first occurs in a map of the spectrum of Bellatrix as a line in a probable new series found by Dr. W. J. S. Lockyer. The second and third lines, given as 4566·8 and 4574·8, occur in a Table of Lines which Sir Norman regards as belonging, with high probability, to gaseous substances which have yet to be discovered.

It will be noticed that the connection existing between the three lines is not there recognised.

In none of the Tables of Wave-Lengths available for reference at the Cape could any satisfactory clue be obtained as to the origin of the lines.

During some experiments made with a view to securing the best elementary line spectrum of oxygen as a comparison spectrum for stars of the β Crucis type, I found that a tube of carbon dioxide gave the best results, being freer from impurities, and giving stronger oxygen lines than any of the oxygen tubes at my disposal. By the use of a jar and air gap in the secondary circuit of the coil the gas was dissociated, and gave the spectra of carbon and oxygen. During use, the carbon dioxide tubes became more vacuous, and, with a view to obtaining a brighter discharge and shorter exposure, I passed the induced current from an 18-inch Apps' coil, using four large jars and an air gap.

Whilst using these electrical conditions, I happened to expose an argon tube marked 2 mm. (pressure), and on developing the photograph was much surprised to find that it, too, gave the well-recognised lines of oxygen. Stronger than these I at once noticed two lines at the green end of the spectrum, which recalled the lines in β Crucis, which were unknown terrestrially, whilst the expected argon spectrum was almost entirely absent.

On comparing the negative, film to film, with one of β Crucis, and allowing for the difference of temperature conditions under which the two negatives were taken, the identity of the three unknown lines in β Crucis with three lines on the argon negative was at once apparent,

* 'Spectra of Southern Stars' (Stanford, 1898), p. 13.

† 'Roy. Soc. Proc.,' vol. 62, p. 60.

and a subsequent photograph of the spectrum of ϵ Canis Majoris, in which the argon tube was used, as stated, as a comparison spectrum, established their absolute identity both as regards position and relative intensity.

It was, therefore, evident that a terrestrial source of the three unknown lines had been discovered, and with the behaviour of the carbon dioxide tube fresh in mind, and the replacement of the argon spectrum by unknown lines and those of oxygen by use of a highly disruptive spark, it is not surprising that an obvious startling explanation as to the nature of the element thus found terrestrially should have suggested itself.

It was at first assumed, erroneously as it afterwards proved, that the origin of the unknown lines lay in the gaseous contents of the argon tube. Four argon tubes in succession gave precisely the same results, viz., the argon spectrum with an ordinary discharge and the unknown lines and oxygen, together with the disappearance of the argon spectrum, as a result of using the jars and air gap. On communicating these results to Dr. Gill, he at once interested himself in the matter, and gave every facility for a further prosecution of the inquiry. He remembered that Professor Ramsay had furnished him with a specimen tube of pure argon, and this tube had not been examined. On trying this tube under the same conditions as the others, it was found to give the argon spectrum under all conditions. Neither the unknown lines nor oxygen made their appearance, even when the most intense disruptive spark available was employed.

The first four tubes had aluminium electrodes, whilst Professor Ramsay's tube had platinum electrodes, and was more vacuous and much shorter.

A pair of aluminium electrodes was then taken from a vacuum tube, and a spark between the metal terminals in air was next examined, with the result that the unknown lines were not found. A line appeared very approximately in the same position as the strongest of the three lines, but this was only one of the numerous air lines, and was due to nitrogen (4552·6 Neovius). Therefore, the electrodes of the argon tubes did not account for the unknown lines.

On a further examination of the negatives, the H and K lines of calcium were recognised in the spectra of the argon tubes subjected to the highly disruptive spark, pointing to the fact that the lime of the glass was being volatilised.

This fact alone might account for the presence of the oxygen lines in the spectra, and the materials of the glass were then suspected as being the origin of the lines under consideration.

A tube of pure helium, kindly furnished to Dr. Gill by Professor Ramsay, was next examined, and, with much surprise, this was found also to behave exactly as the first argon tubes had done.

With an ordinary discharge it gave the pure helium spectrum, but with the highly disruptive discharge the helium spectrum *vanished entirely,* and was replaced by the unknown lines and the spectrum of oxygen. The helium spectrum could be obtained at will by reverting to the ordinary discharge.

This helium tube had platinum electrodes, and these last observations finally banished any idea that the gaseous contents of the tubes or the metallic electrodes could be the origin of the substance searched for, and the conclusion that the glass of the tubes contained the substance sought was now irresistible. Yet in some of the spectra from the helium tube, the H and K lines of calcium were absent when those of oxygen were present, showing that the lime of the glass did not necessarily account for the presence of oxygen.

After various fruitless experiments, sparks were taken between the platinum terminals of a broken up vacuum tube on which still adhered some of the blue fusible glass, commonly used in sealing in platinum wire in glass. The spectrum of this spark in air showed the unknown lines.

Beads of glass made from ordinary soda glass tubing, were then fused on platinum wires, and the spark from these was examined. The unknown lines again appeared. The substance sought was now strongly suspected to be the element silicon. The siliceous diatomaceous earth "kieselguhr" was next used as the most convenient source of silica, and beads of sodium silicate were made by fusing this material with sodium carbonate on platinum wire. The result of the examination of the spark was that the unknown lines were again found. The next step was to replace the kieselguhr by pure rock crystal obtained from the South African Museum by Dr. Gill. Sodium silicate made from the pure rock crystal, also furnished the unknown lines, whilst the sodium carbonate alone failed to give them.

These experiments left little room for doubt that the element sought was silicon. Nevertheless, it was very desirable to confirm the result in another way, by examining the spectrum of a gaseous siliceous compound.

Platinum wires were sealed into the ends of a piece of wide glass tubing, $\frac{5}{8}$ inch internal diameter, the ends of the wires leaving a gap of only $\frac{5}{8}$ inch for the passage of the spark. The tube was also furnished with an inlet and outlet tube for the gas. No capillary tube was used in order to avoid the hot spark coming into direct contact with glass. The tube was then filled with silicon tetrafluoride, and after the gas had been passing for some time, it was sealed off at atmospheric pressure.

An ordinary discharge passed through the gas without jars or air gap gave a banded spectrum of the compound itself.

The disruptive discharge obtained by using four jars and an air gap,

at once gave the unknown lines, which were thus proved to be undoubtedly due to silicon.

This silicon spectrum was not accompanied by that of oxygen, thus proving that it could not be due to any dissociation of the silica of the glass, and that in this case, the gaseous contents of the tube and not the tube itself, furnished the lines under consideration.

Sir Norman Lockyer's papers were then consulted for any reference to the presence of silicon in stars, and it is necessary to refer in some detail to his observations. It is evident that he has used similar powerful disruptive discharges with vacuum tubes, and obtained partial decomposition of the glass, for he says :* "The use of the spark with large jars in vacuum tubes results in the partial fusion of the glass, and the appearance of lines which have been traced to silicium."

Unfortunately he does not give the wave-lengths of the lines thus traced to silicon, and from his statement alone, one would surmise that the origin of the three lines was recognised by Lockyer.

There is evidence, however, *in the same paper* that he cannot have traced the lines in question to silicon notwithstanding the above statement, because, as previously pointed out, Sir Norman regards two of the lines as belonging to gases yet undiscovered, and includes them in a Table of Wave-Lengths of lines due to unknown *gases*.

The other line he also includes as an unknown line in Bellatrix, and Dr. W. J. S. Lockyer places this as a member of a probable rhythmic series due to an unknown substance.

It is a curious fact that Hartley and Adeney, and Eder and Valenta, who alone give us any extended list of lines due to silicon, appear not to have examined the spectrum of this element in the region of the three lines here considered. Their published wave-lengths show only lines in the extreme ultra-violet, and the majority of them are quite outside the region which can be examined by the McClean Star Spectroscope.

Watts's 'Index of Spectra' (Appendix E, p. 21) records a line at 4566 (Salet), but no lines appear corresponding to 4552·79 and 4574·68.

Sir Norman Lockyer† regards two lines at 4128·6 and 4131·4 as the most conspicuous enhanced lines of silicon, indeed these two lines are the only silicon lines he labels Si in his published photographs. Eder and Valenta give 4131·5 and 4126·5 as the least refrangible on their list, and although there is a rather excessive discrepancy in the wave-lengths of one of the lines, they are probably the same pair of lines. They are shown in Lockyer's photographs of the spectra of α Cygni and Sirius‡ and also of α Cygni and Rigel.§

* 'Roy. Soc. Proc.,' vol. 62, 1897, p. 65.
† 'Roy. Soc. Proc., vol. 61, 1897, p. 443.
‡ 'Roy. Soc. Proc.,' vol. 65, p. 191.
§ 'Phil. Trans.,' A (1893), plate 2.

It is a remarkable fact that these three stars, which may be considered as amongst the best examples of silicon stars in the light of the spectrum of silicon hitherto known, *do not show* the three silicon lines which are so prominent in β Crucis, ε Canis Majoris, &c. Scheiner has measured the spectra of all three stars* in this region, but does not record the lines in his Table of Wave-Lengths.

Their absence from the spectra of these stars (as well as the presence of Lockyer's enhanced silicon lines) is fully confirmed by photographs taken here with the special object of searching for the new silicon lines in the best known silicon stars.

This can be readily understood in the light of the experiments with the tube of silicon tetrafluoride.

With the highest disruptive spark, Lockyer's silicon lines 4128·6 and 4131·4 are much enhanced as compared with the lines 4552·79, 4567·09, 4574·68, and it was found possible by suitable exposure to obtain the two enhanced lines without the presence of the other three lines becoming evident.

The latter lines would be much more rapidly obliterated in the absorption spectra of stars, than in the bright line spectrum from the tube, and therefore their absence from certain stars in which the enhanced lines are strong need not occasion much surprise.

In other stars, however, all five lines are present. Lockyer has recorded them in Bellatrix and their presence has been confirmed by photographs of the spectrum of this star taken here. Mr. McClean has measured all five lines in β Crucis where Lockyer's enhanced silicon lines are certainly not so conspicuous as the lines 4552·79 and 4567·09.

The same may be said of ε Canis Majoris in which star the new silicon lines are very prominent, whilst the enhanced lines are very faint.

In the silicon spectrum from the argon and helium vacuum tubes, the enhanced lines noted by Lockyer are by no means so prominent as they are in the silicon spectrum, obtained from silicon tetrafluoride with the intense disruptive spark. It is evident, therefore, that great variations in the relative intensities of the silicon lines occur in stellar spectra, and that such variations can be produced to a certain extent in the laboratory, and these require further investigation.

The behaviour of the silicon lines will give us valuable data for the elucidation of the problem of relative stellar temperatures.

It is clear that if we regard, with Lockyer, the lines 4128·6 and 4131·4 to be the enhanced lines of silicon and their presence, enhanced, to be a criterion of a higher temperature than occurs in stars where these lines are *not* enhanced, it must follow that such stars as α Cygni, Rigel, and Sirius are hotter than Bellatrix, β Crucis, and ε Canis

* Scheiner's 'Astronomical Spectroscopy.'

Majoris. Whereas Lockyer* in his most recent paper "On the Chemical Classification of the Stars" (April, 1899), regards the so-called "Crucian" stars, as at a higher temperature than the "Rigelian" and "Cygnian," and indeed he regards Bellatrix "as a type of the hottest stars, exception being made of ζ Puppis."

Of the other lines recorded by Eder and Valenta† as due to silicon, 3905·4, 3862·5 and 3855·7 are present both in the spectra of the dissociated glass and in the high temperature spectrum of silicon obtained from the silicon tetrafluoride tube.

They are enhanced lines in the latter case, occurring together with Lockyer's enhanced lines in the absence of the three new silicon lines, but they lie outside the region measured by Scheiner in α Cygni, Sirius, and Rigel.

In the Harvard "Spectra of Bright Stars"‡ the two latter lines are however, specially noted in Rigel as 3863·2 and 3856·2 as "conspicuously strong in the ultra-violet," whilst all three are recorded (3905·6, 3863·2, 3856·2) in stars of Groops VI to VIII (Harvard), comprising α Cygni, Sirius, and Rigel. They would thus appear in these stars to accompany the enhanced silicon lines, specially noted by Lockyer, viz. 4128·6 and 4131·4.

The lines 3834·4 and 3836·7 recorded by Eder and Valenta are not present in any of the photographs of silicon spectra, and may possibly be due to impurities.

The lines 3795·9 and 3791·1 recorded by Eder and Valenta are present in all the silicon photographs, but do not become enhanced at high temperatures. There is, however, a third line, approximately λ 3807, not recorded by them, but which appears in all the photographs of silicon spectra. It is stronger than 3795·9 and 3791·1, and does not become enhanced with high temperature. All three lines accompany the three new silicon lines in ε Canis Majoris.

"A Note on the Electrical Resistivity of Electrolytic Nickel." By J. A. Fleming, M.A., D.Sc., F.R.S., Professor of Electrical Engineering, University College, London. Received November 21,—Read December 14, 1899.

The numerical values assigned by various experimentalists for the mass or volume electrical resistivity of certain metals differ very considerably. Some metals are without much difficulty prepared as often

* 'Roy. Soc. Proc.,' vol. 65, No. 416, p. 189.

† Watts's 'Index of Spectra.'

‡ 'Harvard Annals,' vol. 28, Part I, Table 7, p. 23.

as required in a state of such chemical purity, and brought so easily into similar physical conditions as to annealing and density, that determinations made by different observers of their resistivity or specific electrical resistance are nearly identical.

Matthiessen's long-accepted value* for the mass resistivity of copper in the form of hard-drawn wire, viz., 0·14493 standard ohm per metre-gramme, was substantially confirmed by the more recent work of Mr. T. C. Fitzpatrick.† Even the purest electrolytic copper now obtainable in an annealed condition does not show an electric conductivity more than 3 per cent. greater than that of a similar character prepared thirty-five years ago by Matthiessen.

In a research carried out in the years 1892 and 1893 by the author in conjunction with Professor Dewar, careful redeterminations were made of the volume-resistivity at 0° C. of all ordinary metals, taken for the most part in an annealed condition and in a state of great chemical purity.

The values so obtained for the electrical volume-resistivity of silver, copper, gold, aluminium, zinc, platinum, tin, lead, magnesium, and iron agreed fairly well with the values given by Matthiessen, and quoted in most of the treatises on electricity. The resistivity of cadmium, as given by us was, however, considerably larger than that usually stated, although our sample of cadmium was very carefully prepared.

In the case of nickel, the purest nickel we were able to obtain was that prepared from nickel-carbonyl. Dr. Ludwig Mond, F.R.S., was so kind as to furnish us with two tubes of this nickel. It was found, however, to be too brittle to draw into wire, and the operation of melting it would have most certainly introduced impurities. Accordingly, the nickel tube was cut up in the lathe into a spiral, and a resistance coil formed with it which could be used for taking the resistivity ratios at different temperatures, but which was not sufficiently uniform in dimensions to permit its volume-resistivity to be calculated. Hence, in our published results, we took the volume-resistivity of this nickel at 0° C. to be 12,320 C.G.S. units, which is the value given by Everett, said to be derived from Matthiessen's experiments, and simply calculated the resistivity at other temperatures from the experiments given by our own observations with the Mond nickel. About a year ago, however, Mr. J. W. Swan, F.R.S., sent me a sample of nickel wire which he had prepared electrolytically from a hot solution of very carefully purified nickelous chloride. The electrolytic metal was annealed by heating in an atmosphere of hydrogen, after having been drawn into wire through a die.

The nickel wire so prepared and annealed is as soft as a silver wire.

* See 'B.A. Report,' 1864, or 'Phil. Mag.,' 1865.

† *See 'B.A. Report,'* Leeds, 1890, or 'Electrician,' vol. 25, p. 608, 1890.

It had a fairly uniform diameter of about 0·01 inch, and a length of nearly 250 cm.

The mean diameter of this wire was taken with the micrometer microscope at regular intervals of centimetres with the following results:—

Diametral Measurements of Nickel Wire, as read by Microscope at regular Intervals of about 5 cm.

Obs.	Diameter (inches).	Obs.	Diameter (inches).
1	0·0097	26	0 0100
2	0·0097	27	0·0096
3	0·00975	28	0·0098
4	0·0107	29	0·0093
5	0·0100	30	0·0099
6	0·0098	31	0·0102
7	0·0100	32	0·0098
8	0·01015	33	0·00975
9	0·01015	34	0·0095
10	0·0100	35	0·0098
11	0·0098	36	0·00985
12	0·0100	37	0·00965
13	0·01015	38	0·0098
14	0·0099	39	0·00985
15	0·0101	40	0·0101
16	0·0099	41	0·0096
17	0·0099	42	0·0094
18	0·01005	43	0·0098
19	0·0097	44	0·0098
20	0·0098	45	0·00995
21	0·0098	46	0·0098
22	0·00985	47	0·0098
23	0·0101	48	0·0096
24	0 0100	49	0·0099
25	0·01005	50	0·0101

The mean diameter of the wire, as obtained from the above fifty readings, was 0·00985 inch. This was checked by taking the density of the wire with all the usual precautions for obtaining a correct value.

The length, weight in air, and weight in water were determined to be as follows:—

Length of the wire	246·98 cm.
Weight in air	1·1163 grammes.
Weight in water at 18° C. + suspension...	1·00175 ,,
Weight of suspension	0·1000 ,,
Weight of wire at 18° C. in water	0·99175 ,,

From the above observations the density was found to be 8·96 at

18° C. The mean diameter calculated from the length and density is then 0·00997 inch. Hence we have—

Mean diameter from micrometer measurement .... 0·00985 inch.
Mean diameter by specific gravity and length measurement .... 0·00997 „

The mean of both means is 0·00991 inch = 0·02567 cm. This last number was taken as the value of the mean diameter.

The nickel wire was then soldered to thick copper leading-in wires, and wound on a frame of a kind suitable for immersion in liquid air.

A description of this particular kind of resistance coil, which has proved itself to be exceedingly suitable for low temperature work, was given in a paper describing the result of numerous observations on the resistivity of metals at low temperatures, published by Professor Dewar and the present author in 1893.*

A coil having been thus constructed, its resistance was taken at various temperatures in a bath of paraffin oil, and the results are as shown in the table below. The temperature of the bath was measured

Observations on the Resistance of Electrolytic Nickel Wire.

Obs.	Total resistance of nickel wire.	Platinum temperature, *pt.*	Centigrade temperature, C.	Volume resistivity in C.G.S. units.
1	3·4284	1·057	1·232	6974
2	3·7563	18·489	18·29	7641
3	3·9470	28·676	28·32	8029
4	4·1109	36·959	36·51	8363
5	4·3506	48·740	48·21	8850
6	4·5679	58·773	58·23	9292
7	4·5778	59·315	58·78	9312
8	4·7493	67·384	66·85	9661
9	5·0403	80·550	80·11	10253
10	5·2018	88·587	88·25	10582
11	5·3000	93·544	93·29	10782
12	5·3882	95·731	94·88	10961
13	5·2379	89·289	88·97	10655
14	5·0094	78·482	78·02	10190
15	4·7273	66·700	66·17	9616
16	4·5260	57·151	56·61	9207
17	4·3586	48·704	48·18	8865
18	4·1323	37·906	37·44	8406
19	3·9620	31·226	30·83	8060
20	3·8112	21·550	21·30	7753
21	3·4318	2·205	2·35	6981
22	2·090	−80·72	−78·2	4251
23	0·710	−196·89	−182·5	1444

* See 'Phil. Mag.,' September 1893, p. 279, "On the Resistance of Metals and *Alloys at Temperatures* approaching the Absolute Zero."

at the same time by means of a platinum thermometer (P_1) used in previous researches.

The conversion of the platinum temperatures into centigrade temperatures was effected by means of a table drawn up Mr. J. Hamilton Dickson* for this thermometer.

The measurements of the resistance at low temperatures ($-78\cdot2°$ and $-182\cdot5°$) was obtained by measuring the coil in liquid air and melting CO_2 at the Royal Institution Laboratories, by kind permission of Professor Dewar.

The total resistance of the nickel calculated from the above observations is set out in the form of a curve (fig. 1), having resistance as ordinates and temperature as abscissæ.

The curves are given both for temperature in centigrade degrees and temperature in platinum degrees. The curve shows that the resistance is falling steadily downwards to a zero value as the absolute zero of temperature is approached.

In fig. 2 the portion of the curve between 0° C. and 100° C. is shown in an enlarged scale. In fig. 3 the volume-resistivity is set out in terms of temperature.

The results show that the volume-resistivity or resistance per centimetre-cube of this electrolytic nickel at 0° C. is 6935 C.G.S. units. The average temperature coefficient between 0° C. and 100° C. is 0·00618.

The above observations indicate that this electrolytic nickel, as prepared by Mr. Swan, has an exceedingly different and much lower resistivity than that employed for test by Matthiessen thirty-five years ago.

The value of the mean temperature-coefficient of the nickel used in the experiments of Fleming and Dewar in 1893, and prepared by Dr. Ludwig Mond, was 0·00622† between 0° C. and 100° C. It is clear therefore that some extraordinary electrical difference exists between nickel as it can now be produced electrolytically and nickel as it was produced by Matthiessen for his experiments.

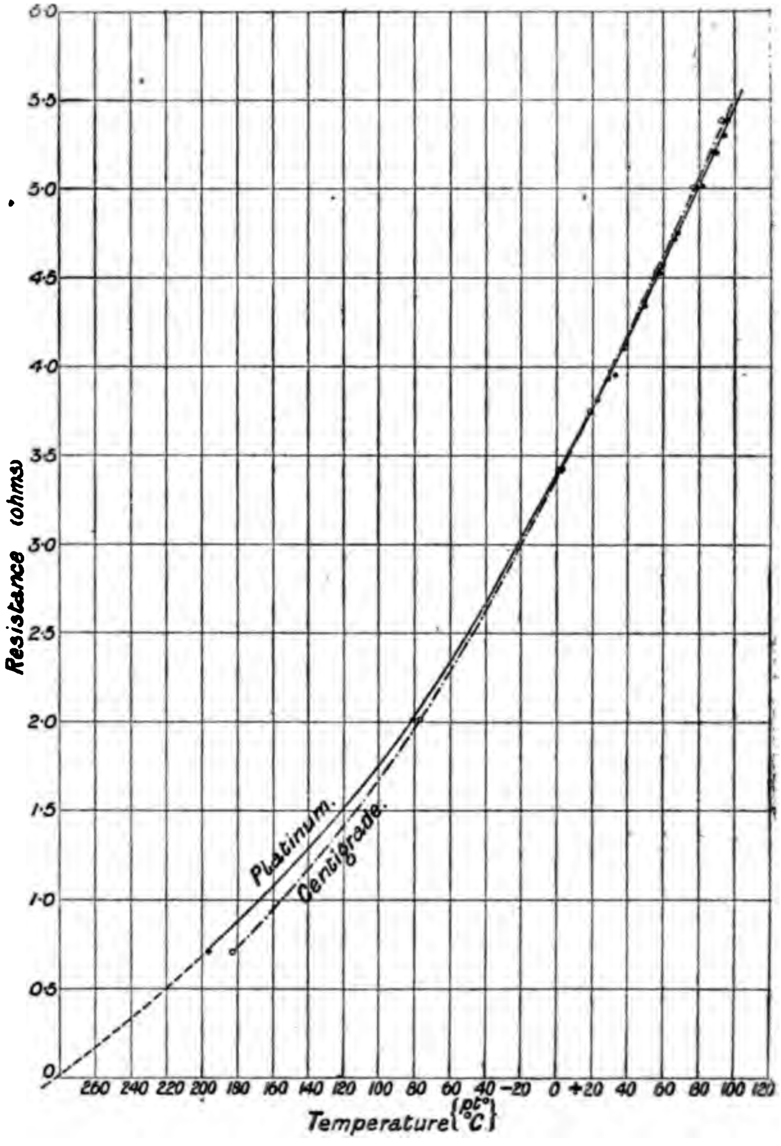
It would be interesting to ascertain if any specimen of nickel known to have been used by Matthiessen for his experiments still exists, and if so, to discover the nature of the impurity (if impurity was present), or at least the physical difference, which caused his nickel to have nearly double the electrical resistivity of that which can now be produced.

I desire to record my thanks to Mr. J. E. Petavel and to Mr. A. Blok for assistance in these experiments.

* '*Phil. Mag.*,' June, 1898.

† '*Phil. Mag.*,' September, 1893.

FIG. 1.—Temperature-resistance Curve for Electrolytic Nickel Wire.



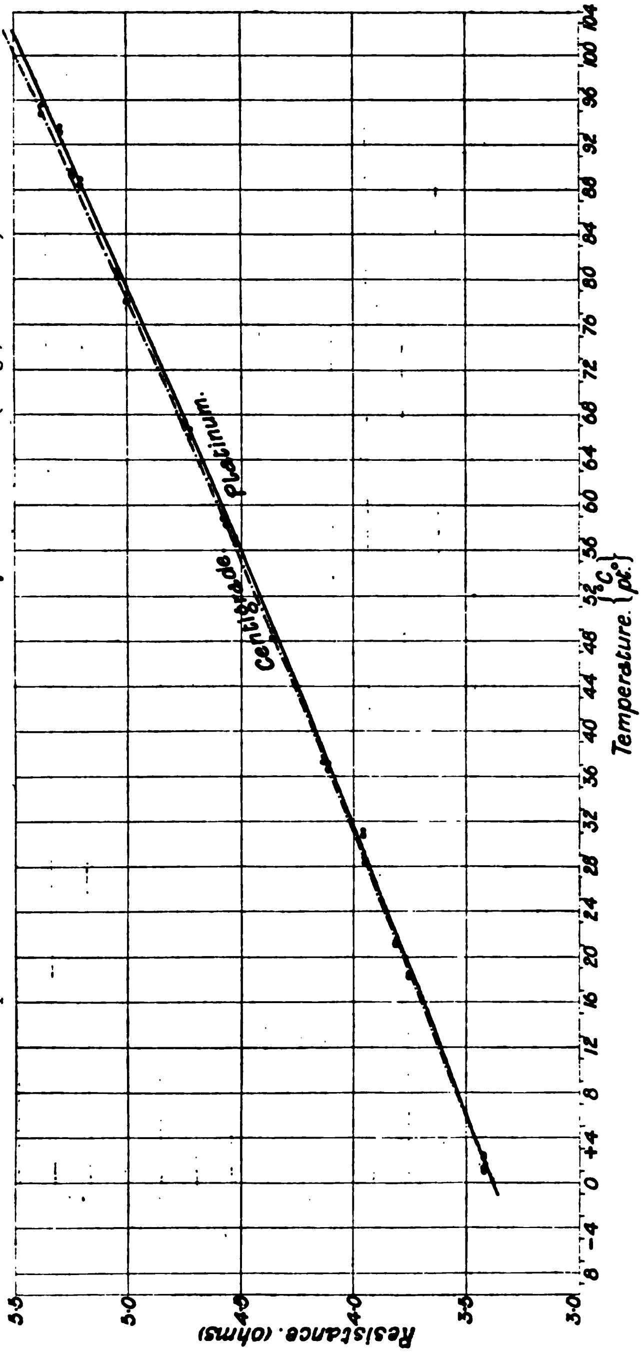
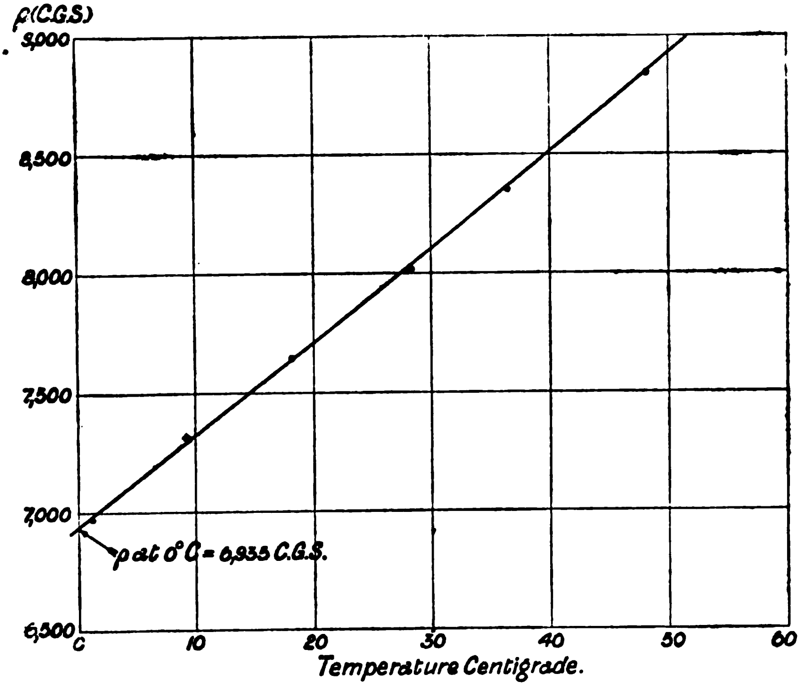


FIG. 2.—Temperature-resistance Curve for Swan's Electrolytic Nickel Wire (Range, 0—100° C.)

FIG. 3.—Electrolytic Nickel Wire (Curve to obtain ρ at 0° C.)



Note added December 6, 1899.—Since writing the above short paper, I have discovered in a paper by Messrs. A. Matthiessen and C. Vogt,* a reference to the sample of nickel with which the present accepted figure for its resistivity was evidently obtained.

This paper is entitled "On the influence of Temperature on the Electric Conducting Power of Iron and Thallium," and its title would not lead a reader to look in it for a reference to the resistivity of nickel.

Messrs. Matthiessen and Vogt therein state that samples of supposed chemically pure nickel and cobalt wires prepared by M. Deville were given to them by M. Wöhler. They measured the resistivity of these samples, but they state that their electrical behaviour gave them reason to believe that this nickel and cobalt were not pure. They give the electrical conductivity of the nickel as 13·11 at 0° C. taking hard drawn silver at 0° C. as 100.

Hence if hard drawn silver has a volume resistivity of 1620 C.G.S. units at 0° C., it follows that Matthiessen and Vogt's value for the resistivity of their sample of nickel would be 12,357 C.G.S. units at

* 'Phil. Trans. Roy. Soc.,' 1863, p. 384.

0° C., which is a number very close to that usually given in tables of electrical specific resistance.*

Matthiessen and Vogt state in this paper, that they hope to be able to prepare pure nickel electrolytically, and obtain a value for its electrical resistivity. I have been unable to discover, however, that they ever carried out their intention. At any rate, the number which they give for the electrical volume resistivity of this nickel of the purity of which they evidently had suspicions, has been accepted for the last thirty-six years as the true value.

"Observations on the Morphology of the Blastomycetes found in Carcinomata." By KEITH W. MONSARRAT, M.B., F.R.C.S.E. Communicated by Professor SHERRINGTON, F.R.S. Received November 22,—Read December 14, 1899.

(From the Pathological Laboratory of University College, Liverpool.)

(Abstract.)

This research was undertaken in order to confirm if possible the observations of Sanfelice, Roncali, and others, on the presence of organisms of the order Blastomycetes in carcinomata, and to study the morphology of the same. The observations have been arranged under four headings:—

1. Isolation by culture.
2. Staining reactions.
3. Histology.
4. Tissue reactions following inoculation.

1. *Isolation by Culture.*—The tumours examined were carcinomata of the breast and uterus; incisions were made with a sterilised knife and scrapings from the edges of these inseminated on to media. Many kinds of media were tried, but a result was obtained only on glucose agar. Wort agar and wort bouillon were subsequently used for sub-cultures; on both the organism grows readily aërobically at 37° C. Sub-cultures on neutral gelatine appear as pale yellow slow-growing colonies without liquefaction of the medium. On neutral agar the colonies have a more marked yellow tinge; they do not appear until

* The numerical values of the specific resistance of nickel given in various tables by different authors are not quite identical, and yet all so far found are stated to be derived from Matthiessen's experiments. Thus, Everett ('C.G.S. System of Units,' 1891 ed.) gives 12,320 C.G.S. units at 0° C. as the value. Landolt and Börnstein give one value equivalent to 12,757 at 0° C. from the ratio of conductivity of nickel to that of mercury, and another equivalent to 12,014 at 0° C., derived from the ratio of the conductivities of hard drawn silver to that of nickel.

after four to five days' incubation at 37° C. There is no growth on acidified gelatine, agar, or bouillon. Neutral bouillon yields a scanty growth after four days' incubation at 37° C.; no scum is formed. On wort bouillon and wort agar the growth is plentiful after twenty-four hours' incubation at 37° C.

On potato at the same temperature there is a characteristic dark brown growth after forty-eight hours. The organism grows more readily anaërobically than aërobically; the growth on potato under the former conditions is white, but turns brown when air is admitted, whereas the growth on agar is brownish-yellow, contrasting with the pure yellow colour of the aërobic growth. These appearances on culture media agree in the main with those described by Sanfelice and Plimmer in the case of the organisms which they have severally isolated.

2. *The staining reactions* of the organism in the tissues were specially studied in order to establish if possible the real characters of the "cancer-bodies" described by many observers. After trial of several methods, the following was decided upon as giving characteristic and distinguishing results. Carmine is first used as a nuclear stain, either in the form of lithium carmine, alcoholic borax carmine (when the pieces of tissue are stained in the mass), or acetic carmine. The last is the only preparation which gives good results with tissue fixed in Flemming's solution. The sections thus stained are placed in a 1 per cent. watery solution of methyl-violet for two minutes, then in a 0·25 per cent. solution of picric acid, washed, dried with filter-paper, and decolorised in clove oil. The methyl-violet is extracted from the plasma and nuclei, but remains in the organisms. The method was proved to give distinctive results in sections of the growths produced by experimental inoculation of the organism isolated.

3. *The morphological characters* of the organism are as follows:—Fresh specimens from cultures are spherical, from 4 to 10 microns in diameter, and in most cases take an aniline chromatin stain diffusely. There is, however, a great variety in the distribution of the chromatin, it is sometimes aggregated to one pole, sometimes divided up at different parts of the cell, and in other cases it is only represented by a few isolated granules. The capsule is delicate. Multiplication in cultures takes place by budding.

In the primary growths produced by intraperitoneal inoculation of the organism, the latter is also in most cases spherical, possesses a delicate capsule, and multiplies by budding. Two peculiarities are, however, to be seen: firstly, in many cases, delicate processes connect adjacent specimens of the organisms; and, secondly, the capsule is often thickened and forms a kind of "halo" round the central deeply staining body of the cell.

In the nodules in lungs, liver, spleen, and kidneys, which are secondary to the growths on the peritoneum, in addition to the forms already described, spore-bearing forms are found. In these the capsule is much thickened, the chromatin of the cell breaks up irregularly, and portions are allowed to escape through dehiscences in the capsule. There is no regularity in the process, no simultaneous division of the cell-contents into a definite number of spores, and no simultaneous shedding of the same. The spores are without capsule when they escape, and are irregular in contour. They stain deeply with chromatin stains, and are finely granular. This method of spore-formation is specially to be noted, as it is entirely unlike the methods described in the case of members of the Saccharomyces class.

4. *Tissue Reactions.*—The animals used for inoculation were guinea-pigs, and the inoculations were made not so much for the purpose of estimating the capacity of the organism for producing cancer as for that of studying the morphological characters of the latter when in the tissues.

The results following intraperitoneal injection of 1 c.c. of a culture forty-eight hours' old are as follows:—The animals showed no symptoms of illness; they were killed at periods from two to six weeks after the injection. On opening the abdomen, the omentum and general peritoneal surface were found to be studded with nodules from the size of a pea to that of a pin's head; of the other organs, nodules are visible to the naked eye in the lungs, liver, spleen, and kidneys. The primary growths on the peritoneum are composed of proliferated endothelial cells; the organisms are present in considerable numbers, some within the cells, but most outside them. In many of the nodules there is some attempt at the formation of a connective tissue capsule, and in others the central parts are broken down.

In the lungs, the nodules are made up of endothelial cells; in each nodule there are organisms present and usually centrally situated.

In the liver and spleen, the nodules are very similar in appearance to the primary omental growths; the origin of the cells composing them is doubtful.

In the kidney, where again the nodules are of endothelial origin, the cells are derived from those lining the Malpighian corpuscles and tubules.

In no case was there any alveolar arrangement of the cells or any appearance resembling the endotheliomata of man.

January 18, 1900.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

The Right Hon. Lord Justice Romer was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Upon the Development of the Enamel in certain Osseous Fish." By C. S. TOMES, F.R.S.

II. "Further Observations on 'Nitragin' and on the Nature and Functions of the Nodules of Leguminous Plants." By Miss M. DAWSON. Communicated by Professor MARSHALL WARD, F.R.S.

III. "On the Innervation of Antagonistic Muscles. Sixth Note." By Professor SHERRINGTON, F.R.S.

IV. "On the Viscosity of Argon as affected by Temperature." By LORD RAYLEIGH, F.R.S.

V. "On the Behaviour of the Becquerel and Röntgen Rays in a Magnetic Field." By the Hon. R. J. STRUTT. Communicated by LORD RAYLEIGH, F.R.S.

VI. "On an Experimental Investigation of the Thermo-dynamical Properties of Superheated Steam." By J. H. GRINDLEY. Communicated by Professor OSBORNE REYNOLDS, F.R.S.

"Upon the Development of the Enamel in certain Osseous Fish." By CHARLES S. TOMES, M.A., F.R.S. Received December 4, 1899—Read January 18, 1900.

(Abstract.)

The author has shown in previous communications to the Royal Society (which are to be found in its 'Transactions') that notwithstanding the fact that in all the vertebrata enamels present tolerably close resemblances in chemical, physical, and histological characters, differences far more considerable than might have been expected exist

in the formative processes. The present communication seeks to establish an additional method of enamel formation, essentially differing from any which has hitherto been described, and whilst the investigation was undertaken in the hope of bridging over the gaps which exist between the methods previously known, it has only partly succeeded in doing so, as the process to be described stands somewhat alone.

The principal point which is set forth in the paper is, that the formative cells of enamel, known as ameloblasts, in all the Gadidæ and in Sargus and Labrus, undergo a preliminary transformation into a reticulated stroma, which is of the full dimensions of the ultimate enamel. During the calcification of enamel, the ameloblasts no longer exist as such, nor do any other cells take their place, but the stroma itself seems able to segregate and properly apply the lime salts required, which make their appearance at that side of the thick stroma which is most distant from the blood vessels.

There have thus been demonstrated four varieties of the process by which enamel is formed, which, although there are gaps not at present bridged over, may perhaps be taken as representing certain stages in the evolution of enamel. These are—

1. Enamels not wholly epiblastic in origin, in which the stroma which is the seat of enamel calcification is furnished by a transformation of the exterior of the mesoblastic dentine papilla, the ameloblasts apparently segregating the lime salts required for its hardening. This is found in the Elasmobranch fishes.

2. Enamels wholly epiblastic in origin, in which the ameloblasts undergo a prior transformation into a stroma of the dimensions of the finished enamel, and themselves disappear. This is the subject of the present communication, and is met with in the Gadidæ, in Sargus and in Labrus, and probably in many other fish.

3. Enamels wholly epiblastic in origin, in which the ameloblasts retain their integrity throughout the whole process. Their extremities are, however, produced into long fibrillar processes, which are traceable far through the calcifying enamel, and these processes are prolongations of the plasm of the cells. This method is found in the enamel of Marsupials, and probably in all similar tubular enamels, such as are found in Hyrax and sporadically among other mammals.

4. Enamels wholly epiblastic in origin, in which the ameloblasts persist throughout the process of calcification. Their free ends are produced into short processes (Tomes's processes), which penetrate but a short distance into the calcifying enamel. This is the ordinary method found in placental mammals.

It will be seen that the last two methods differ in degree rather than in kind, but that the first two stand markedly apart.

Apparently in the Rays there is some sign of the approaching abandonment of the share taken by the dentine papilla, as there is a less

degree of specialisation of its outer portion, but no sign of any transformation of the ameloblasts themselves into any intermediate form of tissue has been observed.

The sudden and entire transformation of the ameloblasts in the Gadidæ may perhaps be correlated with the very early and rapid formation of the enamel, which is formed while there is yet but little dentine calcified.

In mammals enamel formation is a very much slower and more gradual process, and the dentine is always much further advanced towards completion than is the enamel.

A comparison of the various processes now known as occurring in fish, in implacental mammals, and in placental mammals may, in the author's opinion, be taken as finally disproving the idea, which is still entertained by some, that enamel is to be regarded as a sort of secretion shed out from the ends of the ameloblasts; for, imperfect though our knowledge remains in some respects, yet some form of conversion, direct or indirect, of a pre-existent organic matrix is common to all, though as in placental mammals it may be exceedingly small in amount, and the erroneous idea alluded to has proceeded from the study of the process exclusively in placental mammals, in whom its true nature is most difficult to decipher.

"Further Observations on 'Nitragin' and on the Nature and Functions of the Nodules of Leguminous Plants." By MARIA DAWSON, B.Sc. (Lond. and Wales), 1851 Exhibition Science Research Scholar. Communicated by Professor H. MARSHALL WARD, F.R.S. Received December 5, 1899,—Read January 18, 1900.

(Abstract.)

In December, 1898, a paper by the author on "Nitragin, and the Nodules of Leguminous Plants,"* was read to the Royal Society. Since that time the work on this subject has been considerably extended, and a brief summary of the additional results is given below.

Investigations have been made in the following directions :—

A. *Microscopic Observations.*

A comparative study of various points of interest in the anatomy of nodules borne by several genera of different tribes of the order, with special reference to the mode of growth of the nodule organisms within the tissues of the host.

* 'Phil. Trans.,' B, vol. 192, pp. 1—28.

B. *Experimental Work.*

α. Pure cultures of the organisms from *Pisum*, *Desmodium*, and "Nitragin," upon various media, liquid and solid, organic and inorganic, employing the ordinary methods of bacteriology.

β. Direct observations under the microscope of the various stages of growth of colonies, and the formation of bacteroids from straight rods, as seen in hanging drops.

γ. Experiments upon the effect of temperatures above the normal upon the direct infection of pea roots.

δ. Cultures of various genera, representing different tribes of the order, to test the power of organisms proper to one genus to induce tubercle formation upon individuals of other genera or tribes.

ε. Crop cultures in the laboratory greenhouse of peas in sterilised media, with and without inoculation with "nitragin," also with and without a supply of nitrogenous food.

ζ. Crop cultures of peas in ordinary garden soils and in subsoils, in the open, with and without inoculation with "nitragin," also with and without an additional supply of nitrates.

A further study of the morphology of nodules from various genera of the Leguminoseæ, leads to the conclusion that no definite line of distinction can be drawn between genera in which filaments occur in the nodules and those in which they have not yet been observed. Several examples were found of fragmentary portions of filaments in the cells of very young nodules, whilst in older specimens these filaments were quite absent (*e.g.*, *Phaseolus*, *Desmodium*, *Acacia*, and others), suggesting an intermediate stage in the adaptation of the parasite to the special conditions existing in any given host.

During the course of this study, some peculiar anatomical characters have been observed in certain nodules, *e.g.*, the presence of a definite crystal layer in some genera, of apple-green nucleus-like bodies in *Desmodium* and *Robinia*, and of organisms of an unusually large size in *Desmodium*, *Coronilla*, *Psoralea*, and others.

A prolonged study was made of the organisms from *Desmodium gyrans* in particular. Pure cultures were obtained, and from these observations in hanging drops upon bacteroid formation showed that the X and Y forms arise *by distinct lateral branching of the straight rods*.

After twelve to fourteen days' culture, the individual long rods tend to break up into small rodlets, and the branched forms become disjointed in a similar manner. A general study of these organisms and parallel cultures of "nitragin," compared with pure cultures of organisms obtained direct from *Pisum* tubercles, shows that they all alike grow readily on gelatine or agar media containing an extract of *pea stems, asparagine,* and sugar, but very slowly on broth gelatine. *They do not peptonise milk*, but upon potato a watery streak is formed

in about five days; in a liquid medium—pea extract—a thick, zooglœa-like film forms in twelve to fourteen days. The presence or absence of spores in these films is now under investigation. The organisms are aërobic, and may pass through a short motile stage, but the presence of cilia has not been demonstrated. On a medium consisting of silica jelly and a mixture of salts, including ammonium sulphate, abundant growths of the organisms from *Pisum* and *Desmodium* have been obtained; also in hanging drops of silica jelly, colonies of the latter type have grown to 30μ diameter in seven days at 17° C. Further experiments are now in progress in order to test whether these organisms are *per se* capable of (*a*) fixing free nitrogen, or (*b*) converting nitrogen in the form of ammonium salts into nitrites or nitrates; also to determine whether or not the presence of nitrates in the culture medium is directly injurious to the organisms. At a temperature ranging from 24—35° C. (average 30°) a considerable increase in the percentage of direct infections of pea roots was obtained, but at temperatures above 35°, the host plants themselves succumbed after fourteen days. In water cultures only very early stages of infection were observed.

Experiments to determine the action of the organisms proper to one genus upon plants of another tribe or genus suggest that there is probably only one organism capable of producing nodules on leguminous plants, but that in each particular host special physiological conditions exist, to which the organisms become so specially adapted as to make it difficult for successful reciprocal action to take place between plants not nearly allied, though exceptions do occur.

In connection with infection experiments conducted under, as far as possible, sterile conditions, it was determined that fifteen minutes' treatment of seeds with a 0·1 per cent. solution of mercuric chloride before sowing, is without injurious effect upon the seeds, but that a longer action of the solution poisons the embryo. Crop cultures in *sterilised* media give best results when nitrates without organisms are supplied to the plants. The addition of "nitragin" under these conditions is of very little benefit, and if a sufficient supply of nitrogenous food be available, a reduction in the resulting crops ensues when this "fertiliser" is employed.

In *unsterilised* media a small increase in crop may result from the use of "nitragin." The conclusion derived from the various experiments, however, is that the presence or absence of "nitragin" is but one factor in a complex problem, and that at the same time must be taken into account the complicated physical and biological conditions of the soil and atmospheric environments, as well as the symbiotic action of the host plants, in the removal of the products of metabolism from the field of action of the nodule organisms.

"On the Innervation of Antagonistic Muscles. Sixth Note." By C. S. SHERRINGTON, M.A., M.D., F.R.S. Received December 30, 1899,—Read January 18, 1900.

Machine-like regularity and fatality of reaction, although characteristic of spinal reflexes, is yet not exemplified by them to such extent that similar stimuli will always elicit from the spinal animal similar responses. This want of certainty as to response is an interesting difficulty attending the study of spinal reactions. The variation in the responses of the skeletal musculature manifests itself not only in regard to the extent of the movement but also in regard to the direction of the movement.

Some of the factors determining the character of the reactions are factors contained within the stimulus. Important among these is the "*locus* of the stimulus." Thus it has long been known that the direction and other characters of the reflex movement are influenced by the mere location of the stimulus. Nevertheless stimuli identical in all respects, including locality, may evoke reflex movements of widely different, even of absolutely opposite, character. Such differences of response must be referred to differences obtaining at the time in the spinal organ itself. One cause for such differences seems indicated by the following observations :—

The most usual, indeed the almost invariable, primary reflex movement of the hind limb of the spinal dog (and cat), when spinal transection has been performed in the cervical or upper thoracic region, is flexion at hip, knee, and ankle; the limb is "drawn up." This movement can be well obtained by, among other stimuli, the pressing of the pads of the digits upward so as to extend the toe-joints, a stimulus that in some measure imitates the effect upon those joints of the bearing of the foot upon the ground under the animal's weight. Extension as a reflex result from this stimulus is, in my experience, never met with in the homonymous limb in the early time after transection. When a certain period has elapsed, three weeks or more after transection, and shock has largely subsided, it becomes possible to, at times, obtain extension at hip as the primary movement in the homonymous limb. The pressing of the toe-pads upwards, spreading and extending the digits, elicits a sharp movement of extension at the hip, if at the time the initial posture of hip and knee be flexion. If the initial posture of hip and knee be extension, the primary reflex movement excited is, in my experience, invariably flexion. The reflex movement is, it is true, not unfrequently flexion, even when the initial posture is one of flexion; but it is, on the other hand, very *frequently, and especially* preponderantly in certain individual animals, *extension. The passive* assumption of a flexed posture at hip and

knee seems to favour the reflex movement at those joints taking the form of extension. The influence of the posture of the ankle-joint upon the reflex movement at the hip seems negligible, for I have often remarked the reaction at the hip to be unaltered, whether the ankle were flexed or extended, at the time of excitation.

In some dogs, when the spinal transection has been made at the hinder end of the thoracic region, stimulation of the skin of the limb evokes the usual primary flexion at hip and knee wherever the *locus* of the stimulus, except it be in the upper three-fourths of the front of the thigh. Applied in this latter region the stimulus, if the limb be midway between extension and flexion, not unfrequently evokes reflex extension at hip and knee; it does not evoke extension if the initial posture of the limb be extension; but if the limb be, at the time of application of the stimulus, well flexed at hip and knee, reflex extension, instead of reflex flexion, becomes the rule.

In the spinal frog, as in the spinal dog, flexion at hip and knee is the regular reflex response of the musculature of the homonymous hind limb to skin stimuli applied at any part of the surface of that limb. This being true when the initial posture of the limb is, as when pendent, one of extension at hip, knee, and ankle, a difference becomes evident when the initial posture is one of flexion at those joints. In the latter case excitation of the skin within a small gluteal and pubic area, lateral and somewhat ventral to the cloacal orifice, causes extremely frequently not flexion at hip, but extension at that joint. Stimuli (mechanical and chemical) to that area which evoke flexion at the hip-joint when the initial posture of the limb involves extension at that joint, evoke, when the initial posture is flexion, reflex extension at the joint.

These instances seem to indicate distinctly that the direction which a spinal reflex movement elicited by stimuli similar in all respects, including "locality," may take, is in part determined by the posture already obtaining in the limb at the time of the application of the stimulus.

The reaction described above for the spinal frog holds good after previous removal of all the skin from both hind limbs, with the exception of the small gluteal piece necessary for application of the skin stimulus. It would appear, therefore, that the influence of the posture of the limb upon the spinal condition and reaction is not traceable to the nerves of the cutaneous sense-organs of the limbs. There still remain the afferent nerves subserving muscular sense, and connected with the sense-organs in muscles, tendons, and joints. These, as is well known, are largely affected by the various postures of the limb, even by such postures as are passively induced.

"On the Viscosity of Argon as affected by Temperature." By LORD RAYLEIGH, F.R.S. Received January 12,—Read January 18, 1900.

According to the kinetic theory, as developed by Maxwell, the viscosity of a gas is independent of its density, whatever may be the character of the encounters taking place between the molecules. In the typical case of a gas subject to a uniform shearing motion, we may suppose that of the three component velocities v and w vanish, while u is a linear function of y, independent of x and z. If μ be the viscosity, the force transmitted tangentially across unit of area perpendicular to y is measured by $\mu\, du/dy$. This represents the relative momentum, parallel to x, which in unit of time crosses the area in one direction, the area being supposed to move with the velocity of the fluid at the place in question. We may suppose, for the sake of simplicity, and without real loss of generality, that u is zero at the plane. The momentum, which may now be reckoned absolutely, does not vanish, as in the case of a gas at rest throughout, because the molecules come from a greater or less distance, where (*e.g.*) the value of u is positive. The distance from which (upon the average) the molecules may be supposed to have come depends upon circumstances. If, for example, the molecules, retaining their number and velocity, interfere less with each other's motion, the distance in question will be increased. The same effect will be produced, without a change of quality, by a simple reduction in the number of molecules, *i.e.*, in the density of the gas, and it is not difficnlt to recognise that the distance from which the molecules may be supposed to have come is *inversely as the density*. On this account the passage of tangential momentum *per molecule* is inversely as the density, and since the number of molecules crossing is directly as the density, the two effects compensate, and upon the whole the tangential force and therefore the viscosity remain unaltered by a change of density.

On the other hand, the manner in which this viscosity varies with temperature depends upon the nature of the encounters. If the molecules behave like Boscovich points, which exercise no force upon one another until the distance falls to a certain value, and which then repel one another infinitely (erroneously called the theory of elastic spheres), then, as Maxwell proved, the viscosity would be proportional to the square root of the absolute temperature. Or again, if the law of repulsion were as the inverse fifth power of the distance, viscosity would be as the absolute temperature.

In the more general case where the repulsive force varies as r^{-n}, the *dependence of μ upon* temperature may also be given. If v be the *velocity of mean square*, proportional to the square root of the tem-

perature, μ varies as $v^{\frac{n+3}{n-1}}$, a formula which includes the cases ($n=5$, $n=\infty$) already specified. If we assume the law already discussed —that μ is independent of density—this conclusion may be arrived at very simply by the method of "dimensions."

In order to see this we note that the only quantities (besides the density) on which μ can depend are m the mass of a particle, v the velocity of mean square, and k the repulsive force at unit distance. The dimensions of these quantities are as follows:—

$$\begin{aligned} \mu &= (\text{mass})^1\,(\text{length})^{-1}\,(\text{time})^{-1}, \\ m &= (\text{mass})^1, \\ v &= (\text{length})^1\,(\text{time})^{-1}, \\ k &= (\text{mass})^1\,(\text{length})^{n+1}\,(\text{time})^{-2}. \end{aligned}$$

Thus, if we assume

$$\mu \propto m^x \,.\, v^y \,.\, k^z \qquad (1),$$

we have $1 = x+z, \quad -1 = y+(n+1)z, \quad -1 = -y-2z,$

whence $\quad x = \dfrac{n+1}{n-1}, \quad y = \dfrac{n+3}{n-1}, \quad z = \dfrac{2}{n-1}.$

Accordingly $$\mu = \alpha \,.\, m^{\frac{n+1}{n-1}} \,.\, v^{\frac{n+3}{n-1}} \,.\, k^{-\frac{2}{n-1}} \qquad (2),$$

where α is a purely numerical coefficient. For a given kind of molecule, m and k are constant. Thus

$$\mu \propto v^{\frac{n+3}{n-1}} \propto \theta^{\frac{n+3}{2n-2}} \qquad (3).$$

The case of sudden impacts ($n=\infty$) gives, as already remarked, $\mu \propto v \propto \theta^{\frac{1}{2}}$. Hence k disappears, and the consideration of dimensions shows that $\mu \propto d^{-2}$, where d is the diameter of the particles.

The best experiments on air show that, so far as a formula of this kind can represent the facts, $\mu \propto \theta^{0.77}$. It may be observed that $n=8$ corresponds to $\mu \propto \theta^{0.79}$.

When we remember that the principal gases, such as oxygen, hydrogen, and nitrogen, are regarded as diatomic, we may be inclined to attribute the want of simplicity in the law connecting viscosity and temperature to the complication introduced by the want of symmetry in the molecules and consequent diversities of presentation in an encounter. It was with this idea that I thought it would be interesting to examine the influence of temperature upon the viscosity of argon, which in the matter of specific heat behaves as if composed of

single atoms. From the fact that no appreciable part of the total energy is rotatory, we may infer that the forces called into play during our encounter are of a symmetrical character. It seemed, therefore, more likely that a simple relation between viscosity and temperature would obtain in the case of argon than in the case of the "diatomic" gases.

The best experimental arrangement for examining this question is probably that of Holman,* in which the same constant stream of gas passes in succession through two capillaries at different temperatures, the pressures being determined before the first and after the second passage, as well as between the two. But to a gas like argon, available in small quantities only, the application of this method is difficult. And it seemed unnecessary to insist upon the use of constant pressures, seeing that it was not proposed to investigate experimentally the dependence of transpiration upon pressure.

The theoretical formula for the volume of gas transpired, analogous to that first given by Stokes for an incompressible fluid, was developed by O. E. Meyer.† Although not quite rigorous, it probably suffices for the purpose in hand. If p_1, V_1 denote the pressure and volume of the gas as it enters the capillary, p_2, V_2 as it leaves the capillary, we have

$$p_1 V_1 = p_2 V_2 = \frac{\pi t R^4}{16 \mu l}(p_1^2 - p_2^2) \quad \ldots\ldots\ldots\ldots\ldots \quad (4).$$

In this equation t denotes the time of transpiration, R the radius of the tube, l its length, and μ the viscosity measured in the usual way.

In order to understand the application of the formula for our present purpose, it will be simplest to consider first the passage of equal volumes of different gases through the capillary, the initial pressures, and the constant temperature being the same. In an apparatus, such as that about to be described, the pressures change as the gas flows, but if the pressures are definite functions of the amount of gas which at any moment has passed the capillary, this variation does not interfere with the proportionality between t and μ. For example, if the viscosity be doubled, the flow takes place precisely as before, except that the scale of time is doubled. It will take twice as long as before to pass the same quantity of gas.

Although different gases have been employed in the present experiments, there has been no attempt to compare their viscosities, and indeed such a comparison would be difficult to carry out by this method. The question has been, how is the viscosity of a given gas affected by a change of temperature? In one set of experiments the

* '*Phil. Mag.*' vol. 3, p. 81, 1877.
† '*Pogg. Ann.*,' vol. 127, p. 269, 1866.

capillary is at the temperature of the room; in a closely following set the capillary is bathed in saturated steam at a temperature that can be calculated from the height of the barometer.

If the temperature were changed throughout the whole apparatus from one absolute temperature θ to another absolute temperature θ', we could make immediate application of (4); the viscosities (μ, μ') at the two temperatures would be directly as the times of transpiration (t, t'). The matter is not quite so simple when, as in these experiments, the change of temperature takes place only in the capillary. A rise of temperature in the capillary now acts in two ways. Not only does it change the viscosity, but it increases the *volume* of gas which has to pass. The ratio of volumes is θ', θ; and thus

$$\frac{\mu'}{\mu} = \frac{t'}{t} \times \frac{\theta}{\theta'} \quad \text{.........................} \quad (5),$$

subject to a small correction for the effect of temperature upon the dimensions of the capillary. It is assumed that the temperature of the reservoirs is the same in both transpirations.

The apparatus is shown fig. 1. The gas flows to and fro between the bulbs A and B, the flow from A to B only being timed. It is confined by mercury, which can pass through U connections of blown glass from A to C and from B to D. The bulbs B, C, D are supported upon their seats with a little plaster of Paris. The capillary is nearly 5 feet (150 cm.) in length and is connected with the bulbs by gas tubing of moderate diameter, all joints being blown. E represents the jacket through which steam can be passed; its length exceeds that of the capillary by a few inches.

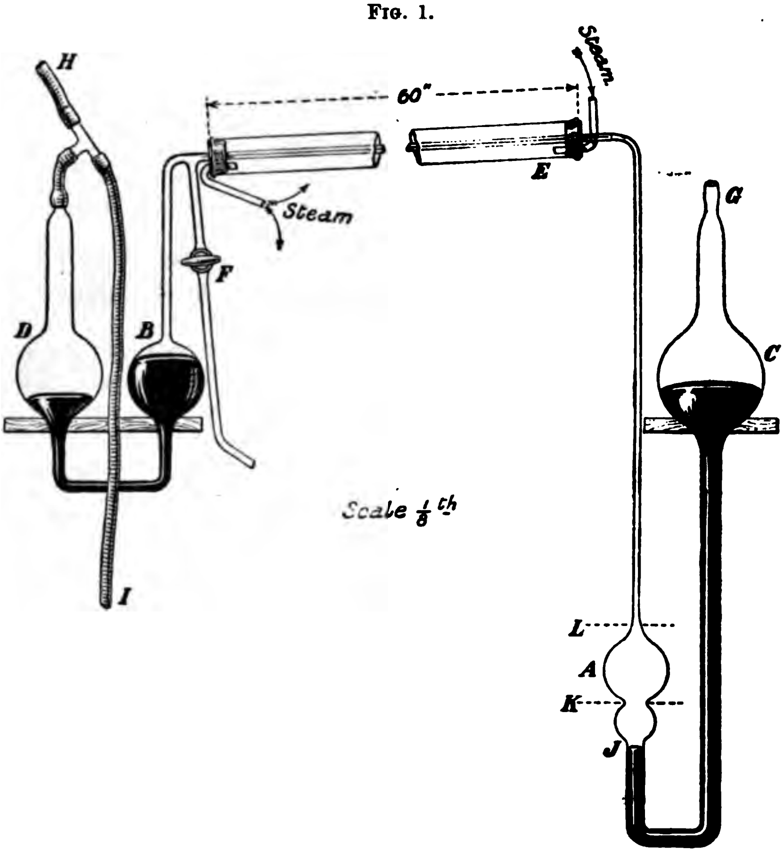
In order to charge the apparatus, the first step is the exhaustion. This is effected through the tap, F, with the aid of a Toppler pump, and it is necessary to make a corresponding exhaustion in C and D, or the mercury would be drawn over. To this end the rubber terminal H is temporarily connected with G, while I leads to a common air pump. When the exhaustion is complete, the gas to be tried is admitted gradually at F, the atmosphere being allowed again to exert its pressure in C and D. When the charge is sufficient, F is turned off, after which G remains open to the atmosphere, and H is connected to a manometer.

When a measurement is commenced, the first step is to read the temperatures of the bulbs and of the capillary; I is then connected to a force pump, and pressure is applied until so much of the gas is driven over that the mercury below A and in B assumes the positions shown in the diagram. I is then suddenly released so that the atmospheric pressure asserts itself in D, and the gas begins to flow back into B. The bulb J allows the flow a short time in which to establish itself before the time measurement begins as the mercury passes the

connection passage K. When the mercury reaches L, the time measurement is closed.

One of the points to be kept in view in designing the apparatus is to secure long enough time of transpiration without unduly lowering the driving pressure. At the beginning of the measured transpiration the pressure in A was about 30 cm. of mercury above atmosphere, and

FIG. 1.



that in B about 2 cm. below atmosphere. At the end the pressure in A was 20 cm., and in B 3 cm., both above atmosphere. Accordingly the driving pressure fell from 32 to 17 cm.

Three, or, in the case of hydrogen, five, observations of the time were usually taken, and the agreement was such as to indicate that the mean would be correct to perhaps one-tenth of a second. The time for air at the temperature of the room was about ninety seconds, and for hydrogen forty-four seconds, but these numbers are not strictly comparable.

When the low temperature observations were finished, the gas was lighted under a small boiler placed upon a shelf above the apparatus, and steam was passed through the jacket. It was necessary to see that there was enough heat to maintain a steady issue of steam, yet not so much as to risk a sensible back pressure in the jacket. The time of transpiration for air was now about 139 seconds. Care was always taken to maintain the temperature of the bulbs at the same point as in the first observations.

There are one or two matters as to which an apparatus on these lines is necessarily somewhat imperfect. In the high temperature measurements the whole of the gas in the capillary is assumed to be at the temperature of boiling water, and all that is not in the capillary to be at the temperature of the room, assumptions not strictly compatible. The compromise adopted was to enclose in the jacket the whole of the capillary and about 2 inches at each end of the approaches, and seems sufficient to exclude sensible error when we remember the rapidity with which heat is conducted in small spaces. A second weak point is the assumption that the instantaneous pressures are represented by the heights of the moving mercury columns. If the connecting U-tubes are too narrow, the resistance to the flow of mercury enters into the question in much the same way as the flow of gas in the capillary. In order to obtain a check upon this source of error the apparatus has been varied. In an earlier form the connecting U-tubes were comparatively narrow; but the result for the ratio of viscosities of hot and cold air was substantially the same as that subsequently obtained with the improved apparatus, in which these tubes were much widened. Even if there be a sensible residual error arising from this cause, it can hardly affect the comparison of temperature-coefficients of gases whose viscosity is nearly the same.

I will now give an example in detail from the observations of December 21 with purified argon. The times of transpiration at the temperature of the room (15° C.) were in seconds

$$104\tfrac{3}{4},\quad 104\tfrac{1}{2},\quad 104\tfrac{3}{4}.\quad \text{Mean, } 104{\cdot}67.$$

When the capillaries were bathed in steam, the corresponding times were

$$167\tfrac{1}{2},\quad 167\tfrac{1}{2},\quad 167\tfrac{3}{4}.\quad \text{Mean, } 167{\cdot}58.$$

The barometer reading (corrected) being 767·4 mm., we deduce as the temperature of the jacket 100·27° C. Thus $\theta = 287{\cdot}5$, $\theta' = 372{\cdot}8$. The reduction was effected by assuming

$$\frac{t'}{t} = \left(\frac{\theta'}{\theta}\right)^x \qquad\qquad (6).$$

With the above values we get

$$x = 1{\cdot}812.$$

As appears from (5), the integral part of x relates merely to the expansion of the gas by temperature. If we take

$$\frac{\mu'}{\mu} = \left(\frac{\theta'}{\theta}\right)^n \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (7),$$

we get $$n = 0{\cdot}812.$$

This number is, however, subject to a small correction for the expansion of the glass of the capillary. As appears from (4), the ratio μ', μ as used above requires to be altered in the same ratio as that in which the glass expands by volume. The value of n must accordingly be increased by 0·010, making

$$n = 0{\cdot}822.$$

The following table embodies the results obtained in a somewhat extended series of observations. The numbers given are the values of n in (7), corrected for the expansion of the glass.

Air (dry)	0·754
Oxygen	0·782
Hydrogen	0·681
Argon (impure)	0·801
Argon (best)	0·815

In the last trials, the argon was probably within 1 or 2 per cent. of absolute purity. The nitrogen lines could no longer be seen, and scarcely any further contraction could be effected on sparking with oxygen or hydrogen.

It will be seen that the temperature change of viscosity in argon does not differ very greatly from the corresponding change in air and oxygen. At any rate the simpler conditions under which we may suppose the collisions to occur, do not lead to values of n such as 0·5, or 1·0, discussed by theoretical writers.

I may recall that, on a former occasion,* I found the viscosity of argon to be 1·21 relatively to that of air, both being observed at the temperature of the room.

* 'Roy. Soc. Proc.,' January, 1896.

"On the Behaviour of the Becquerel and Röntgen Rays in a Magnetic Field." By the Hon. R. J. STRUTT, B.A., Scholar of Trinity College, Cambridge. Communicated by LORD RAYLEIGH, F.R.S. Received January 9,—Read January 18, 1900.

In the current number of Wiedeman's 'Annalen,' an experiment is described by Giesel showing that the Becquerel rays are deflected in a magnetic field. This result is of great interest, on account of the light which it throws on the nature of the rays. Up to the present, the evidence has tended to show that the Becquerel rays were of the same nature as the Röntgen rays, both being capable of penetrating thin metal sheets, of affecting a photographic plate, and of producing ionisation in the surrounding air. Neither could be refracted or reflected; and so far as has yet appeared, neither could be polarised.

These facts seemed to form a fairly strong body of evidence that the two kinds of radiation were essentially similar. But the announcement of the magnetic deflectibility of the Becquerel rays seems to throw doubt on this conclusion. The Röntgen rays, so far as is known, are quite unaffected by magnetic force. Under these circumstances it seemed worth while to make a new attempt to discover such an effect on the Röntgen rays. This attempt I have carried out. It will be best to say at once that the result is negative.

A focus tube was employed as the source of radiation. It was placed at a distance of about 35 cm. from a powerful electro-magnet, and in such a position that the cathode rays in the tube were parallel to the magnetic force due to the magnet. The line joining the oblique anti-cathode to the centre of the magnetic field lay in the plane of the anti-cathode.

A short distance in front of the magnet a wire was placed at right angles to the direction of the rays, and in the plane of the anti-cathode. It was thus at an angle of about 50° to the magnetic force—the same angle as that between the axis of the cathode stream and the anti-cathode. This wire was used to cast a shadow on a photographic plate placed at a distance of 65 cm. on the other side of the magnet.

An exposure was first made with the magnetic force in one direction. The exposure was then stopped, the field reversed, and another exposure given of course without shifting the plate. If then the rays had been appreciably deflected, the photograph should have shown two shadows, either overlapping, or altogether separated.

The rays casting the shadow were those emitted at a grazing angle from the anti-cathode. The reason for using these very oblique rays was that owing to the foreshortening of the anti-cathode, the source was virtually narrower than it would have been, had rays

been used which left the anti-cathode at a greater angle. Thus sharper shadows were obtained, and a smaller magnetic deflection could have been detected.

The tube was arranged with its cathode stream parallel to the magnetic field, so as to avoid any shifting of the source of radiation when the magnet was reversed, owing to an effect of the magnet on the original cathode beam. Such a shifting would have given rise to a spurious effect. The only objection to this was that the shadow-casting wire had to be obliquely placed so as to be in the plane of the anti-cathode. Thus some sensitiveness was lost.

I shall now give an estimate of the smallest deflectibility which could have been detected. The rays traversed a distance of 65 cm. after leaving the magnetic field.

It was estimated that a lateral displacement of the shadow of the wire by 0·02 cm. could have been detected. But the wire was inclined 50° to the resultant magnetic force. Thus the smallest real displacement that could with certainty be detected was $\frac{0{\cdot}02}{\sin 50}$ cm.

The smallest angular deflection of the rays which could be detected would be, in circular measure,

$$\frac{0{\cdot}02}{65 \sin 50} = 0{\cdot}000405.$$

The length through which the rays were exposed to the magnetic force was 8 cm. If in this distance they were bent through the above angle, the radius of curvature would be

$$r = \frac{8}{0{\cdot}000405} \text{ cm.} = 19{,}800 \text{ cm.}$$

The strength of the magnetic field was determined in the usual manner, by observing the throw of a galvanometer when a small coil of known dimensions connected up with it was suddenly withdrawn from the region between the pole pieces. To reduce the results to absolute measure, the throw due to reversal of an earth-inductor in the same circuit was observed.

In this way the strength of the field was found to be

3270 C.G.S.

It is convenient to exhibit the result by giving the maximum field which the experiments indicate as unable to produce a curvature of radius 1 cm.

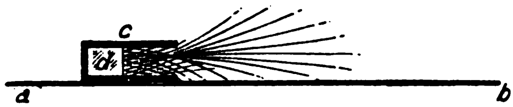
Since a field of 3270 does not produce a curvature of radius less than 19,800 cm., we see that the field required to produce a curvature of radius 1 cm. cannot be less than

$6{\cdot}5 \times 10^{7}$.

Owing to the fact that the magnetic field was *reversed* instead of being merely shut off, the experiment is really of double the sensitiveness indicated above. But, in order to be well on the safe side, it has been thought best to leave this out of account.

For the sake of comparison I have attempted a rough estimate of the amount of the magnetic deflection of the Becquerel rays. The method employed was as follows :—

Fig. 1.



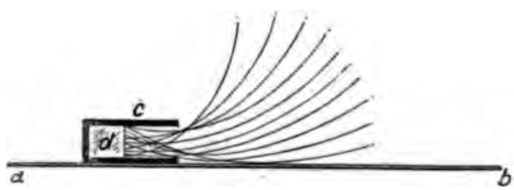
A photographic plate, shown in section at *ab*, was laid on the top of the square pole pieces of a magnet, the magnetic force being perpendicular to the plane of the diagram. The plate was covered with thin aluminium foil ; *c* is a metal capsule filled with the substance *d*, which emitted the rays.*

When no magnetic force was acting, the rays were emitted from the capsule as indicated in fig. 1, some of them striking obliquely on the plate. On development after one hour's exposure, a shadow was obtained beginning at the edge of the capsule *c*, and extending a short distance. The effect gradually tailed off, and at a few cm. distance away from *c* it was inappreciable. When the magnetic force was in such a direction as to bend the rays down into the plate (fig. 2), the

Fig. 2.

shadow extended further. When, on the other hand, the magnet was reversed so as to bend the rays away from the plate (fig. 3), the

Fig. 3.

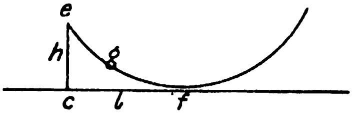


* The substance employed was a preparation from uranium residues, supplied by de Haen, Hamburg.

shadow obtained on development was much shorter, the time of exposure being, of course, in each case the same.

The numerical estimate of the curvature of the rays was obtained from an experiment of the latter kind.

FIG. 4.



Let us suppose that *ec* (fig. 4) represents in section the front surface of the radiating substance, *cf* the surface of the photographic plate.

Let *f* be the place furthest from *c* at which the darkening on the photographic plate was perceptible. Now the rays which reach furthest are those which proceed from *e*, the highest point of the radiating surface, as may easily be seen from geometrical considerations. The rays which reach *f* must consequently proceed from *e*. Rays proceeding from any lower point of the surface will either be bent up so as never to reach the plate at all, or else they will strike it short of *f*. The ray which reaches *f* from *e* will clearly just graze the surface of the plate at *f*.

If r be the radius of curvature, h the distance *ec*, and l the distance *cf*, then

$$h(2r - h) = l^2, \text{ or } r = \tfrac{1}{2}\left(\frac{l^2}{h} + h\right).$$

If then we measure h, the height of the highest part of the radiating surface above the plate, and l the greatest distance to which the darkening of the plate extends, we have data for determining r.

It must be admitted that the measurement of l involves great uncertainty. The image gradually tails off, and any estimate of its length must to a great extent be arbitrary.

The value of r deduced is more uncertain still, since l^2 is involved in calculating it. But, in spite of these objections, the method may, I think, be relied on to give the order of magnitude of r, and that is all that is required, so far as the conclusions which it is here sought to draw are concerned.

In one experiment, the length l was estimated at 2 cm.; h was 0·8 cm. Thus r = 3 cm. approximately.

The strength of the magnetic field, measured as before, was 1680 C.G.S. Thus the field required to produce a curvature of radius 1 cm. is about 5×10^3.

In another experiment, l was 1·8 cm., h was 0·8 cm., and the field 2140. This gives practically the same result as the preceding.

In an experiment described by Professor J. J. Thomson, a beam of cathode rays was bent to a radius of curvature of 9 cm. in a field of 35 units. Thus a field of 315 would have been required to bend it to a radius of 1 cm.

Let us now collect the results obtained, and compare them with this. The field which would be required to produce a curvature of 1 cm. radius would be

For cathode rays	3×10^2
„ Becquerel rays	5×10^3
„ Röntgen rays not less than	6×10^7.

If the Röntgen rays are magnetically deflected at all, it is by an amount less than a ten-thousandth part of that observed in the case of the Becquerel rays.

The magnetic deflectibility of the Becquerel rays cannot but be considered to be a most characteristic property. And the above result appears to make it tolerably certain that the Röntgen rays do not possess this property. It is to be concluded, therefore, that the Becquerel rays are, after all, essentially different in character from the Röntgen rays.

"An Experimental Investigation of the Thermo-dynamical Properties of Superheated Steam." By JOHN H. GRINDLEY, B.Sc., Wh. Sch., Exhibition (1851) Scholar, late Fellow of the Victoria University. Communicated by Professor OSBORNE REYNOLDS, F.R.S. Received April 21, 1899,—Read January 18, 1900.

(Abstract.)

PART I.—*On the Law of Flow of Saturated Steam through Small Orifices.*

In making experiments on the thermal properties of superheated steam obtained by wiredrawing saturated steam, it is essential that certain laws assumed in theory to govern the flow through the orifice should obtain in practice.

Among these laws the only one on which a difference would be expected to exist between experiment and theory, is the law of adiabatic expansion assumed to hold during the flow.

Since such adiabatical flow is not only assumed, but is indispensable in obtaining temperature results in the wiredrawn steam which will enable deductions to be made by theory of the initial dryness of the steam or its thermal condition after wiredrawing, it was found im-

portant to know the circumstances under which adiabatic flow could be experimentally obtained.

Many experimental results have already been given by various experimenters which indicate the laws governing the flow through various types of orifices, and to some extent bear out the theoretical conclusions on the subject, but so far as the author is aware, no experiments have yet been made with saturated steam, showing results which entirely agree with those deduced from theory by assuming adiabatic flow, and hence arose the necessity of making experiments with this object in view.

It appears from the theory, that when the ratio (the lower to the higher) of the two pressures causing the flow through the orifice is diminished below a certain value, the upper pressure being kept constant, the rate of discharge of the steam should be constant. This value of the pressure ratio depends entirely on the law of expansion assumed to hold during the flow. By assuming this law to be represented by an equation of the form

$$pv^n = \text{constant},$$

p being the pressure, v the specific volume of the gas, and n a constant for any particular gas, we can deduce this value of the pressure ratio giving maximum flow in the form

$$\frac{p_2}{p_1} = \left(\frac{2}{n+1}\right)^{\frac{n}{n-1}}.$$

Putting $n = 10/9$ we get for saturated steam expanding adiabatically during its flow through the orifice.

$$\frac{p_2}{p_1} = 0{\cdot}5824.$$

If now the flow of steam be truly adiabatic in an experiment, this particular value of the pressure ratio giving the maximum flow should be actually found by the experiment, and if some other value than this be obtained the law of flow will not then be the true adiabatic one for saturated steam.

Hence the attainment of this particular value of the pressure ratio giving the maximum discharge was made the object of the experimental inquiry here described, since it would follow that the law of expansion through the orifice was then truly the adiabatic law for saturated steam.

To begin with, an orifice was drilled in a piece of thin sheet brass the nature of which should create, if possible, a large deviation from the adiabatic in the actual law of flow through the orifice. Experiments were then made with this orifice placed between a steam chest and a condenser, the weight of steam passing per minute being taken

at various pressures over a wide range of pressure ratio, the upper pressure being kept constant. It was found that the maximum rate of discharge did not occur until the ratio of pressures had fallen to 0·33, a value far below that given by the theory, and indicating a far different law of flow through the orifice than the adiabatic.

As a contrast to this, since the main element in the question appeared to be the conductivity of the substance in which the orifice is made, the later experiments were made with an orifice drilled in a glass plate, the orifice being neither sharp lipped nor smoothly rounded, the lip in the best circumstances presenting a rough chipped edge. Now with such material for an orifice plate, it is evident that any passage of heat between the glass and the steam will be very small, and also that if adiabatic flow is not now obtained, then either there must be a passage of heat between various portions of the glass and the steam in contact with them, or heat must be conducted along the stream of vapour itself, the latter being considered negligible from considerations of gaseous conductivity.

The experiments made with this orifice show a complete agreement between the results of experiment and theory, and that the law of flow through an orifice drilled in a plate of glass, no conditions being attached as to the roundness or otherwise of the lip of the orifice, is precisely the adiabatic law assumed in the theory.

PART II.—*On the Cooling of Saturated Steam by Free Expansion.*

In Regnault's experiments on the total heats of saturated steam under various pressures, the steam was withdrawn upwards from a boiler, allowing any entrained moisture in the steam to be separated by gravity. Saturated steam obtained in any other manner would not necessarily have the same total heat as that obtained by Regnault at the same pressure, and it is therefore of great importance to note that the dryness of the steam in Regnault's experiments was obtained by the simple method of draining suspended moisture from it.

Hence, since the foundation of most of the researches on het thermal properties of steam rests upon Regnault's results, it would be well to accept as a definition of dry saturated steam that condition of steam which is obtained by draining from wet steam any entangled moisture.

In making experiments on the thermal condition of superheated steam obtained by wiredrawing saturated steam, a knowledge of the total heat of evaporation of the steam before wiredrawing is necessary, and as Regnault's tables of the total heats of saturated steam only apply to steam obtained in the above manner, it must also be obtained in the same manner for these tables to apply.

The precise object of this paper is to describe a research on the thermal properties of superheated steam, these properties being deduced

from a knowledge of those of saturated steam already obtained by Regnault.

The temperature and pressure of saturated steam in a steam chest in which a constant supply of steam is kept is taken, the steam is then drawn upwards to an orifice, and, after wiredrawing, its pressure and temperature are again taken, using for the determination of the latter a thermo-electric junction immersed in the steam.

Special precautions were found necessary, and special apparatus designed to prevent losses of heat by radiation from the channel containing the wiredrawn steam, a steam jacket of peculiar construction enveloping this channel completely, and by adjusting the temperature of the jacket to equality with that in the wiredrawn steam, all radiation was effectively prevented from this portion of the apparatus.

Again, communication of heat from one side to the other of the orifice through the substance in which the orifice is made was prevented, and true adiabatic flow obtained through the orifice by drilling it in a piece of plate glass, such as that described in the research on the law of flow through orifices.

During an experiment, the pressure in the steam chest being kept constant, a series of temperature readings at various values of the lower pressure were observed in the wiredrawn steam. By this means a curve showing the cooling of the steam for any degree of wiredrawing from an initial constant pressure could be drawn on a pressure-temperature diagram.

Provided now that the total heat of steam before passing the orifice was known, it would be possible to deduce from these temperature and pressure results the values of the mean specific heat at constant pressure of superheated steam between the saturated condition and the temperature of the wiredrawn steam at any given pressure, and further, the total heat of steam at any pressure and temperature obtained by such wiredrawing, would be known.

Whether the steam was in the same condition before wiredrawing as that obtained in Regnault's experiments was certainly not an easy point to decide. In both cases, however, the steam was obtained by draining any suspended moisture from steam initially wet, but whether this process of drainage always brought the steam into the same condition as to dryness, whatever the degree of wetness originally in the steam, was as yet an open question, which could only be decided by experiment. Accordingly experiments were conducted with saturated steam at a known pressure and temperature in the steam chest, but at different degrees of wetness in different experiments. The results obtained are very important, as the maximum difference of temperature at any particular pressure in the wiredrawn steam which could be found to exist between experiments with different degrees of wetness in the steam in the steam chest was 0·35° F., and generally the differ-

ence could not be distinguished, it being remarked that if the dryness of the steam before passing the orifice had been altered by so little as 0·06 per cent., a difference of 1° F. should have been observed in the temperature of the wiredrawn steam.

It would, therefore, appear that saturated steam at any particular pressure obtained by relieving it of suspended moisture by gravitation has only one condition as to its dryness, and also that steam in this particular condition was obtained both in these experiments and in those of Regnault, and it is therefore taken that the steam before wiredrawing has a total heat given by Regnault's tables of the total heats of saturated steam.

Further experiments were also made to observe the effect of altering the position of the thermo-electric junction in the wiredrawn steam, of the effect of the steam jacket on the temperature of the wiredrawn steam, and of the effect of the velocity of the steam through the apparatus on these temperature readings. The amount of the corrections required for the conduction of heat between various portions of the apparatus and the steam was also calculated, but on account of the precautions taken these were generally found to be negligible. The method of fixing the absolute temperature of the wiredrawn steam should be here mentioned, as it is a point of great importance, on account of the difficulties attending the accurate measurement of the temperature. In the experiments the thermometer was used merely as a scale to compare the temperature of the wiredrawn steam with that of saturated steam under a known pressure flowing through the same portions of the apparatus with about the same velocity, the fixing of the temperature being again dependent on Regnault's tables of the pressure-temperature relation of saturated steam.

The final results obtained show clearly that within the limits of temperature obtained by wiredrawing saturated steam at temperatures varying from 240° to 380° F., the condition of the steam known as a perfect gas was not obtained, even when the wiredrawing was continued to 3 lbs. or 4 lbs. per square inch absolute pressure; and further, that between the same temperatures and between pressures of 2·5 lbs. and 195 lbs. per square inch, there was not found any indication of a constant value of the specific heat at constant pressure in the superheated steam. The specific heat at constant pressure was found to increase with temperature, the mean specific heat at atmospheric pressure between the temperatures 230·7° and 246·5° being 0·4317, and between temperatures 295° and 311·5° the mean specific heat was 0·6482.

As regards the variation in the value of the specific heat at constant pressure of superheated steam with the pressure, it appears from an examination of the results obtained at about the same temperature but under different pressures, that if any such variation in the

specific heat exists it will be very small compared with the variation with temperature, such examination indicating that the value of the specific heat is sensibly independent of the pressure.

The law of cooling followed by the wiredrawn steam is slightly different from that obtaining in many other gases, viz., that the fall of temperature varies directly as the difference of pressure. The rate of cooling was found to diminish with increase of initial temperature.

The curves showing the pressure-temperature relations of the superheated steam wiredrawn from definite initial pressures, seem to follow for a short distance the law of boiling points, and the experiments show that this coincidence always exists in saturated steam, and may well be mistaken for evidence of wetness in the steam.

Tables showing the fall of temperature with pressure in the wiredrawn steam, of the total heat of the steam under certain pressures and temperatures, and of the mean value of the specific heat at constant pressure of superheated steam at definite pressures and between definite temperatures, accompany the paper.

PART III.

In this portion of the paper the two properties of steam deduced directly from the experimental figures, viz., the specific heat K_p and the cooling effect $\delta\theta/\delta p$ or c, are more directly considered. In the first place, the cooling effect c is found to be inversely proportional to $\tau^{3\cdot 8}$, where τ is the absolute temperature.

It is then shown that the following formula

$$\frac{\partial}{\partial p}(K_p) = -\frac{\partial}{\partial \tau}(cK_p)$$

is capable of strict proof from thermodynamical principles, the interpretation of the formula being that the variation of K_p with the pressure at constant temperature is equal to the variation of the product cK_p with the absolute temperature at constant pressure, but of opposite sign.

Applying this to steam when superheated, it has been shown in Part II of the paper that the variation $\frac{\partial}{\partial p}(K_p)$ is zero to the degree of accuracy to which the experiments have been taken. It follows, therefore, from the above formula, that the variation $\frac{\partial}{\partial \tau}(cK_p)$ should equal zero; hence, the values of the product cK_p have been tabulated for different pressures and temperatures, and so far as the results go, it is clearly shown that the product cK_p is an absolute constant, which means that the variations $\frac{\partial}{\partial \tau}(cK_p)$ and $\frac{\partial}{\partial p}(cK_p)$ are both zero.

Since the variation $\frac{\partial}{\partial\tau}(c\mathrm{K}_p) = 0$, it is possible to integrate at once for the case of superheated steam Thomson's formula for the cooling effect c, which may be written

$$\frac{d\tau}{\tau} = \frac{dv}{v + c\mathrm{K}_p},$$

the resulting equation being

$$\frac{v + c\mathrm{K}_p}{\tau} = \mathrm{A},$$

when A may be a function of the pressure. This equation has been used to find the specific volumes of superheated steam under various conditions of pressure and temperature, the value of A being deduced from known data in the saturated condition of the steam.

The calculated specific volumes, the accuracy of which depends solely on the experimental results obtained in the research, are compared with those obtained experimentally by Hirn, the results in general agreeing very well.

It is also of interest to notice that in any gas in which K_p does not vary with the pressure, the product $c\mathrm{K}_p$ must also be independent of the temperature in that particular gas, since the equation

$$\frac{\partial}{\partial p}(\mathrm{K}_p) = -\frac{\partial}{\partial\tau}(c\mathrm{K}_p)$$

must be satisfied identically, and hence the equation

$$\frac{v + c\mathrm{K}_p}{\tau} = \frac{dv}{d\tau}$$

must be immediately integrable for the gas in the form

$$\frac{v + c\mathrm{K}_p}{\tau} = f(p).$$

"Researches in Absolute Mercurial Thermometry." By the late S. A. Sworn, M.A. Communicated by H. B. Dixon, F.R.S. April 21,—Read June 15, 1899.

(Abstract, prepared at the request of the Council by Arthur Schuster, F.R.S., December, 1899.)

The experimental portion of this work consists of the careful comparison of six thermometers, with the object of studying the effects of capillarity, and in the second place of obtaining a comparison between thermometers made of English flint glass with those of French "verre dur" or Jena normal glass, and therefore indirectly with the hydrogen scale.

The instruments employed consisted of a Tonnelot "verre dur" thermometer, to be referred to as No. 4976, an English flint glass (No. 711,179) by J. J. Hicks, two normal thermometers (Nos. 2218 and 2219) of Jena 16^{III} by Gerhardt of Bonn, and two calorimetric thermometers Nos. 2220 and 2221 of Jena 16^{III} by Gerhardt, with a range from $-2°$ to $25°$ C.

The Tonnelot instrument is divided on the transparent stem into tenth degrees, and is cylindrical in the bore. The other thermometers have enamelled backs, and are divided on the stem into half millimetres. At the time the latter instruments were obtained elliptical bores were the only ones procurable, but care was taken that the bore was not unduly flattened, and was smooth in contour. The author considers the readings taken with these thermometers to be quite trustworthy. The ratio of the major to the minor axis of the bore was about 2 for the Jena glass thermometers and 3 for 711,179. In each case the bulb (without enamel) was fused to the stem. Ampoules were avoided in all the instruments.

The calibration corrections were obtained in the usual way, a micrometer being used to measure the ends of the thread. The reduction was made by the Neumann-Thiessen method.

All readings other than those for calibration were made with a telescope magnifying eighteen to twenty-four times, the eye-piece of which was provided with a micrometer scale by Zeiss. With the aid of this eye-piece, which serves to further subdivide the thermometer divisions, the readings agreed to 0·005 mm. Several readings were always taken, generally three for zero readings, six for the indications in steam, twenty-one for coefficients of external pressure, fifty-four for coefficients of internal pressure; twenty-seven of zero and fifty-four in steam for the fundamental internal correction, and ninety of comparison and eighteen of zero on each instrument during the comparisons. The

probable error of the separate results for the various constants is about 0·001° C.

The Constant of Capillary Depression K *and the Coefficient of External Pressure.*—It is usual to determine the pressure coefficient by suspending the thermometer in a tube, the pressure within which can be rapidly changed from atmospheric pressure to one of a few centimetres of mercury. A sudden diminution of pressure p causes a fall in the indications of the thermometer, and in the absence of capillary effects δ/p would measure the so-called coefficient of external pressure. But the fall of thermometer being accompanied by a change in the shape of the meniscus, the readings before and after the change of pressure are not directly comparable. If the meniscus is normal at high pressure (which can be realised in the experiment, by arranging for a slowly rising temperature), we must add to the reading at the low pressure a certain constant K, which represents the difference in the readings of a thermometer between a rising and falling thread. Hence $(\delta + \mathrm{K})/p$ will be the corrected coefficient of external pressure (β). If ten sets of observations are made, with different changes of pressure (p_1, p_2) giving different falls of the thermometer (δ_1, δ_2), we may put

$$\beta_e = \frac{\delta_1 + \mathrm{K}}{p_1} = \frac{\delta_2 + \mathrm{K}}{p_2},$$

and hence
$$\mathrm{K} = \frac{p_1\delta_2 - p_2\delta_1}{p_2 - p_1}, \qquad \beta_e = \frac{\delta_2 - \delta_1}{p_2 - p_1}.$$

In the actual observations the changes of temperature which take place between the readings must, of course, be allowed for, and the equations only hold if these changes are so slow that at the low pressure the actual temperature has not overtaken the apparent temperature at the moment the reading is taken. Previous observers not being interested directly in the quantity K, have arranged their experiment so that the readings of low pressure were only taken after a time sufficient to allow the rising temperature to have its effects, so that the thread was rising in all observations. Mr. Sworn, on the other hand, wishing to determine K and β_e simultaneously, had to arrange the experiment so that at low pressures the hydrostatic pressure in the bulb was the same as with a falling thread.

The following table gives an idea of the consistency of the results obtained :—

Thermometer.	Temperature.	p_2.	K.
2220 p_1 = 525 mm.	0°	654 655 636 544 445 363	0·1005 mm.
	19·5°	668 659 654 561 424	0·1243 mm.
	20·5°	669 664 574 414	0·1070 mm.

Mean K = 0·111 mm. ± 0·005.
= 0·0065° ± 0·0003.

The value of K for the various thermometers was found to be as follows:—

Thermometer.	K.
2220	0·0065° ± 0·0003
2221	0·0098° ?
2218	0·0087° ± 0·0006
2219	0·0104° ± 0·0013
4976	0·0051° ± 0·0004
711179	0·0105° ± 0·0004

Mr. Sworn concluded from his results that K is a constant not affected by a change in the rate of rise in temperature, and not appreciably different in different parts of the tube, if the average value of K over a space of several millimetres is always taken.

The Fundamental Interval and the Coefficient of Internal Pressure.—The zeros were determined by plunging the thermometer into a mixture of finely pounded ice and distilled water. Samples of ice were frequently prepared from distilled water, which had for some time been kept in a partial vacuum of 50—100 mm. Norwegian ice was also used, and within the limits of experimental error was always found to give the same results as the specially prepared ice. The purity of the ice was invariably controlled by testing for chlorides, by the Nessler test, and by evaporation to dryness in a platinum basin. In order to be sure

that the two varieties of ice would give the same results, control determinations were made with Nos. 2220 and 2221, the indications of which could be relied upon to show differences exceeding 0·001° C. The apparatus for taking the zeros did not differ materially from that generally used and described by Guillaume.

The thermometer was plunged into the ice within one or two minutes after removing it from the hypsometer, whilst the bulb was still at 40—50°. The thermometer was held vertically in the hand until the mercury had fallen sufficiently for the bulb to be immersed with safety into the ice. 5—10 mm. of the stem above 0° C. were exposed to the ice, adjustment to the vertical made, the thermometer raised so that the image of the meniscus was just clear of the ice, and the readings taken. The stem was always well tapped.

The indications of thermometers at the temperature of saturated steam were investigated in a form of rotary hypsometer which presents some slight difference from that used at the Bureau International. The difference consists in an improvement of the position and construction of the manometer which measures the pressure excess of the steam. In the Breteuil instrument the manometer keeps its vertical position while that part of the hypsometer which holds the thermometer may be placed in either a vertical or horizontal position. This construction renders it necessary for the manometer opening to be placed at some distance from the thermometer bulb, the two being separated by a narrow passage through which the steam has to pass. The manometer will, therefore, register too high a pressure. To correct for this the steam in passing into the condensers when it is at atmospheric pressure, is forced through an exactly similar passage, so that the hypsometer pressure may be assumed to be half way between the pressure indicated by the manometer and the atmospheric pressure. Mr. Sworn gave up the convenience of having the manometer in a fixed position and secured thereby greater certainty in measuring the actual steam pressure at the thermometer bulb. Arrangements had to be made, of course, for the manometer to turn so as to keep the water column vertical when the tube is placed in the horizontal position. Two manometers were used, one connected with the inner chamber holding the thermometer, and the other with the outer steam jacket, but no difference in pressure could ever be detected. In order to prevent the formation of troublesome water drops in the manometers, short and wide glass chambers were interposed between them and the steam. It was thus ascertained that the pressure excess of the steam could be kept within 0·02 mm. of mercury.

A distillation of mercury was avoided by leaving the ast two degrees of the thread unexposed until it was thought that the depressed zero had attained a constant position. Tapping was always resorted to, but the author has been unable to satisfy himself that it makes any

difference for any of the instruments. By comparing the observations of the boiling point made with the vertical and horizontal thermometers, the coefficient of *internal* pressure may be determined, as in the vertical position the hydrostatic pressure of the mercury acts on the bulb. The internal pressure correction (β_i) is connected with the external pressure correction (β_ϵ) by the relation

$$\beta_i = \beta_\epsilon + \kappa,$$

where κ is the compressibility of glass. The following table gives the comparison between the observed and calculated value of β_i; in degrees per millimetre pressure of mercury. The column headed h shows the length of the mercury column between the boiling point and the centre of the bulb.

Thermometer.	β_i (calculated).	β_i (observed).	h.	κ (assumed).
4976	0·0001229	0·0001241	63·8	0·0000154
711179	0·001159	0·001172	57·3	0·0000127
2218	0 000785	0·000804	56·7	0·0000143
2219	0·000825	0·000878	59·2	0·0000143

The observed values of β_i were obtained by dividing the differences in the observed reading (horizontal—vertical position) by the height of the mercury column above the centre of the bulb. No correction was made for the fact that when the column of mercury was raised from the horizontal to the vertical position, the thread descended, and the reading therefore corresponded to one taken with a falling temperature, while in the horizontal position the reading corresponded to one taken with rising temperature. Mr. Sworn concluded from the good agreement between the observed and calculated values of β_i that the effect of capillarity is somehow eliminated in these observations.

Direct observations were made on this point. If the hypsometer is observed at an angle θ to the horizontal, the readings should differ by the quantity K according as the thermometer is brought into its position from the horizontal, or from the vertical. The observed differences are, however, for the most part *nil*, and in any case a mere fraction of K. If, further, the thermometer is gradually raised from the horizontal position, the observed differences in the readings should be expressible in the form $h\beta_i \sin\theta - \text{K}$, where h is the total height of the mercury column, but in reality they are well expressed by leaving K out of account. The author remarked on this point:—

"The effect of capillarity on the advancing or receding columns is unquestionably liable to compensation, either by vibration or by

momentary alterations in temperature or pressure not registered by the manometers. I am personally inclined to think that we are dealing in the hypsometer with steam under what I might term oscillatory conditions of temperature and pressure, the effect of which is to reduce *all* the steam indications of the thermometer to what they would be with a receding meniscus. Within narrow limits the mercury may advance along the tube, but of necessity there will subsequently be a capillary force erected which will, within the same narrow limits (viz., K), prevent its return.

Comparison of Thermometers.—The apparatus in which the comparisons were conducted consisted of a cylindrical tank surrounded, except at the top, by a jacket kept at constant temperature, by a circulation of water heated in a thermo-regulator. The capacity of the tank was 5 litres, and it could be heated or cooled independently, and its temperature set a few degrees above or below that of the jacket. The contained water would then heat or cool at a definite and constant rate. The upper part of the tank has two plate glass sides let in parallel to one another and at right angles to the reading telescope. Two series of comparisons were made. In the first the normal thermometers were compared at 20°, 40°, and 60° C. At each temperature the instruments were compared, two at a time in six pairs, the zeros being taken immediately after the second set of readings for each pair. In the second series the calorimetric thermometers, 2220 and 2221 were also utilised, the former in closed series with the normals. These comparisons were made at intervals of 5° from zero to 55° and at 80°. It is not necessary to refer further to the results obtained with the calorimetric thermometers, as it was found that the water in the comparison tank was slightly different at different levels according as the tank was at a temperature higher or lower than that of the atmosphere. The bulbs of the calorimetric thermometers being placed at different and varying levels as compared with the bulbs of the standard thermometer, the results were vitiated, but this source of error did not affect the comparison of the standards. In the reduction of observations Mr. Sworn reduced all readings to a *falling* meniscus. Assuming that the actual observations at the freezing and boiling points of water are those corresponding to a falling thread, he adds to such reading in the comparison the constant K previously determined by him. From the result of his investigations, Mr. Sworn drew the conclusion that there is no systematic difference between the indications of the *verre dur* and the Jena 16^{III} thermometers, and that the flint glass thermometers give indications which are practically identical with those of the hydrogen thermometer.

[The details of the observations are deposited in the Archives of the Society.]

Note on the above Paper. By ARTHUR SCHUSTER, F.R.S.

Received January 4, 1900.

Mr. Sworn's investigations raise some questions of importance in the behaviour of mercury thermometers. The irregularities which are observed in the behaviour of the mercury thread of a thermometer while descending have led observers to take accurate measurements only in a slowly rising temperature. To avoid inconsistencies, the standard temperatures ought also to be measured under conditions which secure the normal formation of the mercury meniscus, which is that of a rising thread. At the temperature of boiling water it is supposed that this can be done by stopping momentarily the flow of steam, so as to lower the temperature before bringing the mercury thread to its final position. At the freezing point a difficulty has always been felt about the influence of effects of capillarity, and there is no doubt that this is the weakest point at present in the accurate measurement of temperatures with mercury thermometers.

Mr. Sworn's investigations led him to conclude that if the fall of a thermometer is slow (*i.e.*, when the meniscus travels its own diameter in about one minute), the fall is regular, and not a series of disjointed steps. The difference in the readings of a falling and rising thermometer being, according to him, a constant (K), which can be determined by the method described in his paper, it should be possible to reduce readings taken with a rising meniscus to readings with a falling thermometer by simply adding K to the reading.

Mr. Sworn's contention was that this should always be done, because the freezing point is approached from above, and the boiling point also, according to him, corresponds to a measurement taken with a falling thread. His observations on the behaviour of thermometers in the hypsometer are of considerable importance, but some confirmation is required, because Guillaume describes an experiment which is not in agreement with Mr. Sworn's conclusion, that the difference in the readings between a rising and falling thermometer disappears when the instrument is suspended in steam. On the contrary, Guillaume determines the amount of the difference by observations in the hypsometer, and states it to be between 0·002 and 0·003° with the standard Tonnelot thermometers. I am inclined to think that the truth lies between the two extremes, and that the effects of capillarity are still appreciable in the steam, but decidedly smaller than at lower temperatures. I am led to this conclusion through my observations at the freezing point with Tonnelot thermometers, which have always made me think that Guillaume must have underrated the effects of capillarity. But even granting for a moment that there is no effect of capillarity in the hypsometer, I should not be inclined to accept Mr. Sworn's explanation of the fact, which is that the temperature of

the steam is slightly fluctuating, and that when it is accidentally high the temperature rises, but when it is low, stiction prevents the thread from falling, so that the ultimate effect is to make the thermometer indicate too high by an amount equal to K. There is no evidence in support of such a fluctuation. It is at least equally probable that stiction is actually of smaller importance at the higher temperature, where the distillation of mercury, which is known to take place from the free surface, must assist the formation of the normal meniscus. Mr. Sworn's method of reducing thermometric observations depending on the complete disappearance of K at the boiling point, cannot there fore be accepted without further evidence, but the matter is one well worthy of careful investigation. Mr. Sworn was perfectly right in saying that the three readings, viz., freezing point, temperature, and boiling point, ought to be taken under like conditions of the meniscus, but the proper way of accomplishing this is to alter the usual practice of fixing the zero by substituting a method similar to that of determining the freezing points of solutions. If the water is first slightly undercooled, and then brought to the proper temperature by the introduction of a few ice crystals, a great improvement in zero point determinations would be effected.

Mr. Sworn's comparisons between thermometers of different composition were carried out with great care, and may be considered reliable, as far as the instruments used are concerned, but it is not quite certain in how far different thermometers purporting to be made of the same glass may differ. Thus the majority of the Jena glass thermometers carefully studied at Berlin showed a difference of over 0·01° at 50° when compared with the French hard glass,* but one instrument agreed throughout its range with the latter, while another differed in the other direction. Marek, at Vienna, did not find any systematic difference between the French and Jena glass, and Mr. Sworn's thermometers also show a practical coincidence. Mr. Sworn's evidence that his instruments were really made of the 16$^{\text{III}}$ Jena glass rests on the assurance of the maker (Gerhardt, of Bonn), but the anomalous behaviour of two of the Berlin thermometers leaves a doubt as to how far blowers are careful to guard against accidental mixing up of different sorts of glass. It is not possible, moreover, to compare directly the result of Mr. Sworn's comparison with that of other observers, on account of the difference in the method of reduction, but, as far as I can see, the discrepancy would have been greater if Mr. Sworn had reduced his observations in the way adopted at Sèvres and Charlottenburg. The same remark applies with greater force to Mr. Sworn's reduction of the flint glass indications to those of the hydrogen thermometer. He uses Chappuis's numbers for the relation between the French hard glass and hydro-

* 'Zeits. f. Instrumentenkunde,' vol. 15, p. 438 (1895).

gen scales. But Chappuis's numbers only apply when the glass thermometers are treated and read in the way in which he treated them in his comparisons. That is the great advantage in using the Tonnelot thermometer. It is not an absolute but an intermediate standard. It is immaterial whether Chappuis's method of treating a glass thermometer can be improved upon by adopting a different way of obtaining the zero, or by making corrections for effects of capillarity. It is sufficient to know that consistent results can be obtained if the thermometers are always treated in the same way, and whether that way is good or bad, it is the only one which can be used, if we wish to refer the temperature measurement to Chappuis's hydrogen thermometer. For this reason, and also because we have not at present any guarantee that flint glass thermometers agree sufficiently in composition to give identical results, Mr. Sworn's conclusions cannot at present be accepted as final. Mr. Chree's experience* tends in the direction of indicating differences in the behaviour of different thermometers nominally made of the same glass.

January 25, 1900.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Mathematical Contributions to the Theory of Evolution.—On the Law of Reversion." By Professor KARL PEARSON, F.R.S.

II. "On the Mechanism of Gelation in Reversible Colloidal Systems." By W. B. HARDY. Communicated by F. H. NEVILLE, F.R.S.

III. "A Preliminary Investigation of the Conditions which determine the Stability of Irreversible Hydrosols." By W. B. HARDY. Communicated by F. H. NEVILLE, F.R.S.

IV. "On the Effects of Strain on the Thermo-electric Qualities of Metals. Part II." By Dr. MAGNUS MACLEAN. Communicated by LORD KELVIN, F.R.S.

V. "On the Periodicity in the Electric *Touch* of Chemical Elements. Preliminary Notice." By Professor JAGADIS CHUNDER BOSE. Communicated by LORD RAYLEIGH, F.R.S.

---

* 'Phil. Mag.,' vol. 45, p. 216 (1898).

"On the Mechanism of Gelation in Reversible Colloidal Systems."

By W. B. HARDY, Fellow of Gonville and Caius College, Cambridge. Communicated by F. H. NEVILLE, F.R.S.

Received January 12,—Read January 25, 1900.

Speaking generally, colloidal matter occurs in three conditions:—

(1) As fluid mixture, colloidal solutions, or sols, as Graham called them;

(2) Solid mixtures of fluid and solid, the gels; and

(3) Solids, such as dry silica or dry glass.

The property of forming gels is not possessed by all those mixtures which have been classed as colloids. Some only form slimes, which even to the point of actual drying retain the fluid property of flowing. Serum albumen and water is an instance.

Those which form gels fall into two well-defined classes, according to whether the change from the sol to the gel is, or is not, reversible by a reversal of the conditions which produce it. Silica and water may be taken as the type of the latter, gelatine and water of the former. When a hydrosol of silica forms a hydrogel, the latter is "insoluble." To this class belong hydrosols of metallic hydrosulphides and oxides. A hydrosol of gelatine sets to a hydrogel by lowering the temperature; the process is however reversed when the temperature is again raised. As the inner mechanism of the gelation of the hydrosols must differ in the two cases, since in the one irreversible, in the other reversible molecular aggregates are formed, I propose to distinguish the processes by different names. The production of an insoluble gel I will call "coagulation," of a soluble gel "setting." This nomenclature is in accordance with general usage.

Temperature is the most potent factor in determining whether a mixture which forms reversible gels is in the sol or gel state. There is also a limiting concentration of the solid below which the gel state is impossible at any temperature.

"Setting," as a rule, follows on a fall of temperature. Caseine, the chief proteid of milk, furnishes, I believe, the only known exception. In the presence of a small quantity of free alkali it forms a hydrosol. When a small quantity of a solution of calcium chloride or nitrate is added to this, a mixture is produced which forms a hydrogel on warming, and which reforms the hydrosol on again cooling.*

PART I. *Reversible Colloids.*

Systems containing two or three components occur, that is, binary or ternary mixtures. The binary system, agar-and-water, was studied

* Sydney Ringer, 'Journal of Physiology,' vol. 11, p. 464, 1890.

at considerable length; and initial experiments were made with the ternary systems, gelatine-water-alcohol, gelatine-water-mercuric chloride, and agar-water-alcohol. These mixtures are homogeneous when heated, but, on cooling, there occurs a division into two fluid phases. In the binary systems and in the ternary system agar-water-alcohol, the conjugate phases have approximately the same refractive index. In the ternary system, gelatine-water-alcohol, the refractive index of the one phase differs so much from that of the other as to permit of direct microscopical investigation of the form of the surface which separates the phases. For this reason I propose to treat this ternary system first.

The Ternary System—Gelatine-water-alcohol.

When 13·5 grammes of dry gelatine are dissolved in 100 c.c. of a mixture of equal volumes of absolute alcohol and water, a system is produced which is clear and homogeneous at temperatures above 20°. As the temperature falls below this limit a clouding occurs, which I find to be due to the appearance of fluid droplets which gradually increase in size until they measure 3μ. On cooling further these fluid droplets become solid, and they begin to adhere to one another. In this way a framework is built up, composed of spherical masses hanging together in linear rows which anastomose with one another. The framework, therefore, is an open structure, which holds the fluid phase in its interstices. The macroscopical result of the change is the conversion, with falling temperature, of the fluid into a loose gel. The droplets can be readily separated from the interstitial fluid by the help of a centrifugal machine.

The phenomena above described undoubtedly depend upon the separation of a homogeneous mixture into two phases owing to a fall of temperature. Each phase contains water, alcohol, and gelatine, and the system may be described as a conjugate composed of a fluid solution of gelatine in a mixture of water and alcohol, and a solid solution of water with a trace of alcohol in gelatine. Both phases, however, are fluid within a small range of temperature. The surface of separation of the two phases is curved, and at first discontinuous, and owing to the small size of the droplets it is very large.

When the gel is heated the two phases again mix to form a clear fluid. Owing however to the fact that the droplets adhere to one another, they tend to fuse as temperature rises, so as to form irregular masses of viscous fluid, which are separated from the other phase by a surface of no particular shape. The irregular form of this surface, and the ease with which it is modified by any chance slight currents, show that at this stage the surface tension between the two phases must be exceedingly slight. In order to simplify the description I shall call that phase which separates as small spheres the "*internal phase*."

The concentration of the gelatine in the mixture exerts a very remarkable influence upon the configuration of the hydrogel. When it is present in large quantity the internal phase is less viscous and of smaller gelatine content than the external phase, and on cooling it is the external phase which becomes a solid solution. The effect of increasing the proportion of gelatine above a certain amount is therefore very striking—it, so to speak, turns the system inside out, so that the gel is composed of a continuous framework of solid solution, out of which are hollowed spherical spaces filled with fluid. The general mechanical properties of the gel, built on this plan, naturally differ very much from those of a gel with a small proportion of gelatine, which consists of an open framework of solid holding fluid in its interstices.

A mixture of gelatine, water, and alcohol is a ternary mixture which resembles a mixture of benzene, acetic acid, and water. In each there are two immiscible substances and a common solvent. The immiscible substances are gelatine and alcohol in the one case, and benzene and water in the other, while the common solvent is water in the former and acetic acid in the latter case. In both systems the solubility of the immiscible substances in the common solvent varies widely. Thus acetic acid and water, and water and alcohol, mix readily with rise of temperature; while acetic acid and benzene and water and gelatine mix freely only when the temperature is above 15° in the former case, and above 40° in the latter case. Duclaux's researches* show that in ternary mixtures having this last characteristic the distribution of the constituents in the two phases varies widely with variations in the composition of the whole mass.

These different characters are illustrated by the following figures, which give the amount of gelatine present in grammes per 100 c.c. They are, however, only approximate for the solid phase, owing to the difficulty in separating it completely from the fluid phase.

	Total mixture.	Internal phase.	External phase.
T. 15°	6·7	17·0	2·0
	13·5	18·0	5·5
	36·5	8·5	40·0

The temperature at which the internal and external phases in this ternary system mix was found to be altered by altering the ratio of the masses of the components. Increasing the proportion of either of the two immiscible components, alcohol or gelatine, was found to raise the temperature, while an increase in the proportion of the common solvent water was found to lower it.

The curvature of the surface which separates the phases was found

* 'Ann. de Chim. et de Phys.,' série 5, tome 7, 1876, p. 264.

not to be constant for a given mixture. The internal phase formed droplets which were large or small according to whether the mixture was cooled slowly or rapidly. Thus with a mixture containing 13·5 grammes gelatine per 100 c.c. the droplets (of solid solution) were very regularly 3μ in diameter when about 20 c.c. was allowed to cool slowly in air. Cooled rapidly, however, in an ether spray, the droplets were so minute as barely to be visible with a magnification of 400 diameters. The effect of the rate of cooling is the same when mixtures with a large gelatine content are used, and when, therefore, the internal phase is a fluid solution at ordinary temperatures. When cooling is very rapid the droplets are excessively minute; when it is slow they may be as large as 10μ in diameter (gelatine 36·5 per cent. of the mixture). One can therefore make the general statement that *the more slowly the division into two phases occurs the smaller and less curved is the surface of separation.*

The effect upon the structure of the rate at which a fresh condition is imposed upon the system is manifested in a very striking way when an already formed gel is cooled. The experiments upon the effect of temperature on the composition of the two phases in the case of the hydrogel of agar show that when heat is added to or taken away from the system the balance of the phases is altered, water, and perhaps agar, passing from the one to the other. It might be expected that this would take place solely by the passage of material across the surface which separates the two phases. The study of the ternary mixtures, however, makes it clear that a new approximate equilibrium may be reached in two distinct ways.

When a portion of the hydrogel of gelatine-water-alcohol is cooled slowly from 16° to, say, 3° or 4°, one can see with the microscope no change beyond an alteration in the size of the droplets already present, that is to say, the fresh (approximate) equilibrium is attained by exchange across the surface which separates the phases. But if the cooling is rapid, say a fall of 10° in a few minutes, a secondary system of small droplets appears.

In all the mixtures which I examined these were formed in the external phase. Thus, when the concentration of gelatine in the whole mass was low, it was the fluid phase which underwent a division into secondary phases; when it was high, it was the solid phase. To put this fact in a general way, one can say that *when the hydrogel is exposed to a rapid fall of temperature the phase which lies on the convex side of the surface of separation undergoes division into two secondary phases.** When the temperature is again allowed to rise these secondary phases fuse before there is any obvious change in the relation of the primary phases.

* The formation of the secondary phases therefore occurs in that one of the primary phases which is under the lower hydrostatic pressure.

When once formed the phases have considerable stability. If the droplets are composed of a solid solution one may, by the addition of water, cause them to increase to relatively vast dimensions without their being destroyed, as they increase in size their refractive index approximates more and more to that of the external phase until finally they are lost sight of. The addition of alcohol, however, once more brings them into view and causes them to shrink. Owing to this stability once a configuration is established one has to far overstep the conditions of its formation in order to destroy it. This would account for the remarkable hysteresis observed in reversible gels. Thus a 10 per cent. solution of gelatine in water sets at 21° and melts again at 29·6°, and solutions of agar in water set at temperatures about 35° and melt at temperatures about 90°. Similarly with the ternary mixtures. In one holding about 35 per cent. gelatine, the internal and external phases separate at 20°, but they mix again only at 65°. When water is added to a ternary mixture so as considerably to swell the droplets the system is unstable, and the two phases mix at once when it is mechanically agitated.

The properties of the ternary system: alcohol, gelatine, and water are the following:—

i. Below a certain temperature it exists in two phases separated by a well-defined surface. The temperature at which the separation occurs depends upon the relative proportion of the components in the mixture. Increasing the proportion of gelatine raises it; as does also an increase in the proportion of alcohol. An increase in the proportion of the common solvent, water, however, lowers the temperature at which the biphasic character develops.

ii. Both phases are at first fluid; with further fall in temperature one becomes solid.

iii. The surface of separation is curved and discontinuous. In some cases, strictly as a secondary change, the discontinuous masses of the internal phase become continuous with one another.

iv. The more slowly the two phases are established the less is the surface which separates them both in extent and in curvature.

v. The solid solution phase is formed sometimes on the concave, sometimes on the convex side of the surface of separation. The former happens when the proportion of gelatine is small, the latter when it is large.

It follows from the last (v) of these properties that a hydrogel may be built on two very different plans. It may consist of a solid mass containing spherical fluid droplets, or of solid droplets which, by hanging one to the other, form a framework in the spaces of which

fluid is held. These two types present important mechanical peculiarities. The former is firm and elastic, and it maintains its structural integrity even under high pressure. The latter is much more brittle, and manifests a tendency to spontaneous shrinking, which is due to a continuous increase in the surface of contact or possibly union between droplet and droplet. These gels with an open solid framework therefore specially manifest that property of spontaneous shrinkage to which Graham applied the term "synæresis."

In the building of a hydrogel of the second type two distinct events occur. The first is the separation of droplets, which rapidly become solid; the second is the linking of these droplets together to a pattern so that they build a framework throughout the fluid phase. The first is the separation of a homogeneous mass into two phases; the second is a phenomenon akin to the grouping of particles which are suspended in a fluid. It is probable that these two events are not directly connected with one another.

Binary Mixtures (Agar-water).

I know of no binary reversible system in which the optical characters of the two phases differ sufficiently to permit of direct microscopical investigation of the surface of separation. It is, however, easy to prove that in such a system as agar and water the property of gel building is dependent upon the appearance of two phases.

The agar which was used was prepared from commercial agar as follows. The strips were suspended in a large volume of distilled water for twenty-four hours; the water was then drained off, and a large part of the water absorbed by the strips was squeezed out by a powerful press. The strips were again suspended in distilled water, and again drained and squeezed after forty-eight hours. This washing with distilled water was continued for some weeks. The strips were then melted and the hydrosol filtered, and the filtrate allowed to set. The clear hydrogel so obtained was sliced and suspended for a further period of weeks in many changes of distilled water. In this way a colourless gel was obtained free from all foreign diffusible bodies. It was not found necessary to take precautions against micro-organisms. With the removal of the salts the agar ceased to afford them a suitable nidus.

Effects of Pressure upon the Hydrogel of Agar.

When gels containing 1 to 3 per cent. solids* are broken up and slightly squeezed by hand a fluid exudes. In order to collect this fluid a screw press was made use of. The gel was cut into pieces, which were wrapped in fine cotton canvas which had been completely freed

* By this is meant 1 to 3 grammes per 100 grammes.

from soluble substances by treatment for months with hot and cold distilled water. The packet was then pressed in a screw press, and the large yield of fluid collected. When the fluid ceased to flow, the solid which remained in the canvas was removed to a stoppered vessel.

The fluid was found to be a solution of agar. This was proved by evaporating some after it had been thrice filtered to a small bulk, when it was found to set to a typical clear gel. The results of the study of the ternary systems give sufficient grounds for defining the expressed fluid as a solution of agar in water, and the solid which remains in the canvas a solution of water in agar. The effect of the composition of the gel and of temperature upon the distribution of the water and the agar in these two phases was determined. The percentage composition was arrived at by drying a known weight of each, and assuming that the residue was entirely composed of agar. The results which were obtained lie far outside of any error which could have been introduced by this assumption when one considers the pains which were taken to free the gels from foreign bodies. Further, in every case an examination of the dry residue was made in order to prove that it was composed of matter capable of forming with water a typical agar gel.

Experimental Difficulties.

The method used to separate the two phases, though at first sight crude, was found to be the most effective. The great error to be avoided is the blocking of the canvas pores by a mass of the solid phase, so that, instead of the true fluid phase, one really expresses fluid which has been forced through a membrane (pressure filtered). Owing to this error, a press, which I had specially made, and in which the piston drove the gel directly down on to a disc of canvas, proved quite useless. Very great force was necessary to express a fluid which was found to be almost pure water.

To succeed, it is necessary to avoid direct or great pressure. The masses of gel are loosely placed in a long canvas packet, which is then deformed by pressing the ends together. The pressure necessary to yield abundant fluid is now quite small, for the solid framework of the gel is destroyed by being rubbed against the canvas, and is reduced to fine particles, while the fluid easily makes its way through the coarse pores of the canvas. Raising the pressure always expresses a fluid poor in agar, while with slight to moderate pressure the concentration of the expressed fluid, as tested by determining the solids in the yield at different stages, remained fairly constant, but always with a slight decrease as time went on.

The expressed fluid was filtered before the solids were estimated; this was found to lower the amount of solid to a very slight extent.

The following figures illustrate the variation in the composition of the fluid as the gel becomes more completely expressed:—

Successive equal quantities of Expressed Fluid contain Dry Agar in 100 c.c. T. = 14°.

Experiment I.	Experiment II.
0·12	0·11
0·14	0·12
0·1	0·09

The mechanical pressure used to separate the phases will modify their composition by deforming the surface of separation. This error cannot be estimated.

The Influence of the Ratio of the Masses of the Two Components upon the Composition of the Phases.

Two portions of a fairly concentrated gel were taken. To one part water was added to dilute it, and both were then heated to 100°, and equal portions of each were poured into two glass stoppered cylindrical vessels of identical shape, make, and size. The two vessels were then set aside to cool.

After forty-eight hours samples were cut from different levels in each gel, and used to determine the percentage composition.

Five hundred and eighty grammes of each of the gels were then expressed. The results were as follows:—

	Agar in 100 grams of the gel.	Expressed fluid.		Solid solution.	
		Volume.	Agar.	Volume.	Agar.
	grammes.	c.c.	per cent.	c.c.	per cent.
T. = 18°	1·1	440	0·1	140	4·7
	3·3	230	0·14	350	5·6
In another experiment—					
T. = 15°	1·6	—	0·12	—	—
	2·2	—	0·14	—	—

Thus an increase of the concentration of agar in the mixture produces an increase in the concentration of the agar in both phases. An explanation of this relation is suggested, and discussed later.

Effect of Temperature upon the Composition of the Phases.

This was determined by running a large mass of the hydrosol into a number of glass vessels of the same shape and size. Each vessel held 600 c.c. of the hydrosol. They were close stoppered and allowed to cool to the room temperature. After forty-eight hours they were placed in chambers of known temperature, where they were kept for five to seven days before the contents were subjected to pressure. In these experiments, as is obvious, the internal changes are those which follow on raising or lowering the temperature of the hydrogel from the air temperature. In other experiments the hydrosol was cooled down to, but not below, the temperature of observation. This distinction is important, because it was found that the composition of the phases varied for a given temperature according to whether that temperature was the lowest of a descending series or the highest of an ascending series. This is shown clearly in the two curves AB AB', fig. 1. The arrows indicate the direction, ascending or descending, of the changes of temperature.

No. I.—Agar content of Mixture 1·6 per cent.

	Temperature.	Agar in expressed fluid.	Agar in solid.
		per cent.	per cent.
Ascending series	14°	0·14	—
	33	0·29	—
	50	0·80	—
Descending series	14	0·14	—
	33	1·10	—

No. II.—Agar content of Mixture 2·23 per cent.

	Temperature.	Agar in expressed fluid.	Agar in solid.
		per cent.	per cent.
Ascending series	13°	0·12	4·7
	36	0·25	5·0
Descending series	5	0·09	3·0
	13	0·12	4·7
	36	0·47	3·2

Putting on one side for a moment the different effect of an ascending or a descending temperature change, these experiments show that (1) a hydrogel of agar is a structure form of a more solid part and a fluid, and (2) each of these two phases is a mixture of agar and water,

(3) the composition of the phases is dependent to a lesser degree upon the ratio of agar to water in the entire mass, to a greater degree upon the temperature.

While recognising as fully as possible that only an approximation to the actual composition of the two phases at different temperatures is obtained by these experiments, it is obvious that they afford reliable information on two points. These are, firstly, the marked "lagging" action or passive resistance to change offered by the system agar-water. The difference in composition of the phases according to whether any given temperature lies in an ascending or a descending series shows how slow the system is in reaching final equilibrium.* Secondly, the experimental results seem to me to indicate pretty clearly the general form of a part of the concentration temperature curve. I give the curve as it appears from the figures in Experiment II. AB and CD are the curves for the system—solution of agar in water, solution of water in agar, and vapour. If they correctly represent the general form of the curve, then, by the theorem of Le Chatelier, it follows that the change from the system solution of water in agar and vapour to the system solution of water in agar, solution of agar in water, and vapour will be accompanied by a liberation of heat when the change takes place along the isotherms from 5° to ± 20°, and by an absorption of heat when the change is along the isotherms ± 20° to 35°, while the change from the system solution of agar in water and vapour to the system of two solutions and vapour will always be accompanied by absorption of heat.

I have not established this deduction experimentally, but it finds a considerable amount of support in the following facts. When water is allowed to dissolve in pure dry agar at 14°, a considerable amount of heat is given off. 1 c.c. of dry agar in coarse powder added to 10 c.c. of water gives a rise of more than 6°,† while control experiments with carefully dried finest graphite or sand gave a rise of temperature of 0·15° and 0·17° respectively. Wiedemann and Lüdeking‡ also found

* The systems salicylic acid and water, and thorium sulphate and water are perhaps comparable cases. The former readily yields two fluid phases which, however, are throughout in labile equilibrium (Bancroft, 'The Phase Rule,' p. 105). In the case of the latter system supersaturated solutions can be obtained over a wide range of temperatures, and even in presence of the stable hydrates it is often hours or days before equilibrium is reached (Bancroft, *loc. cit.*, p. 54, or Roozebrom, 'Zeits. f. Phys. Chem.,' vol. 5, 1890, p. 198). The lagging action in colloids which is so markedly shown by van Bemmelen's researches into the effect of time on the hydrogel of silicic acid, ceases to be extraordinary when one remembers that one of the phases is a solid. Gels reach equilibrium much more rapidly than does, for instance, a bar of metal in which the reaction velocity is so slow that final equilibrium may never be reached.

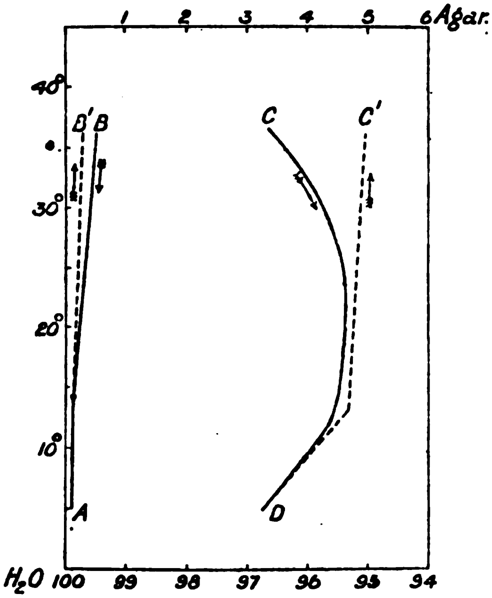
† The mercury in the Beckmann thermometer was driven beyond the scale into the upper reservoir.

‡ 'Ann. der Phys. u. Chem.,' N.F., vol. 25, 1885, p. 145.

that when dried gelatine absorbs water heat is liberated, but when gelatine saturated with water is dissolved in water heat is absorbed.

I have verified the general form of the curve AB in a way which eliminates all the errors due to the expression of the fluid phase from the gel. A cylindrical column of gel 15 cm. high was divided by two vertical cuts at right angles into four equal pieces. Four stoppered glass vessels were taken of the same size and shape, and in each one of the pieces was placed and just covered with water. Two of the bottles were kept at 14° for a week, and two at 44°; the water in both was found to have dissolved some of the agar, and to

Fig. 1.



contain per 100 grammes of the solution 0·50 gramme and 0·12 gramme of dry agar respectively.

The curves AB, DC continued upwards will meet at some point which marks the consolute temperature for agar and water. I have attempted to fix this point by observing the changes in the intensity of the beam of polarised light scattered normal to the ray when parallel light is passed through a gradually cooling hydrosol. The observations, which are still in an initial stage, have so far failed to fix the point.

The study of ternary systems under the microscope makes it probable that as the curves AB, DC are continued upward they reach a point

beyond which the equilibrium is no longer between a fluid solution and a solid solution, but between two fluid solutions.

The first worker to regard gelation as being due to the formation of two phases, one fluid and the other solid, was van Bemmelen.* He has given a suggestive discussion of the formation and structure of gels, based chiefly upon the manner in which amorphous material is precipitated from a solution, and he is led to the conclusion that "coagulation or the precipitation of a gel from a solution seems to be a similar phenomenon; a desolution (Entmischung) which forms, not two layers completely separated from one another, but

"1. A framework of a material which is in a more or less transitional state between fluid and solid, and which presents those special properties to which the term colloid is applied.

"2. A fluid which is enclosed within this framework."

Van Bemmelen, however, does not consider that these two parts can be considered as two phases in the sense of the phase rule, since there is no sharp line between them,† and he therefore concludes that the phase rule cannot be employed to elucidate the phenomena. This opinion is based upon a study of the equilibrium between the water content of various gels and the vapour pressure, so patent and thorough as to give it very great weight. The curves of the equilibrium points are gradually bending lines if the dehydration of the gels is sufficiently slow, but if dehydration is relatively rapid there is a sudden change of direction (fig. 2) when the water content is very much diminished (1 to $2H_2O$ to $1S_1O_2$).

It is possible that the form of these curves does not necessarily depend upon the absence of a clear separation between the fluid and the solid portions of the gel. When one considers how small is the mutual solubility of silica and water and how slight therefore the influence which a given mass of silica is likely to exert upon the vapour pressure of even a relatively small mass of water, it is probable that the form of the curve is determined more by the operation of secondary influences, such as capillary tension, which depend on the structure of the gel, than upon the direct interaction of silica and water. Capillary tension would tend to lower the vapour pressure‡ with which the gel is in equilibrium to a greater and greater extent as the spaces in the solid framework of the gel became smaller and smaller with the decrease in the water content. The tendency to reduce the surface

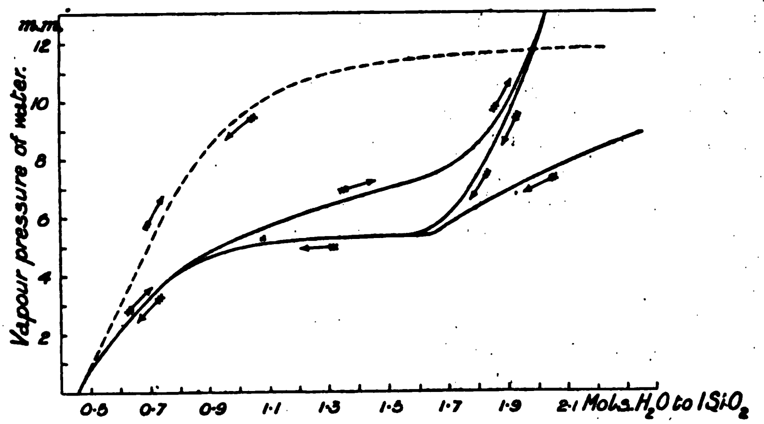
* 'Zeits. f. Anorg. Chem.,' vol. 18, 1898, p. 20.

† 'Zeits. f. Anorg. Chem.,' vol. 18, 1898, p. 121.

‡ The vapour pressure which van Bemmelen measured is that of the free surface of the gel. It is analogous to that at the open ends of a number of capillary tubes filled with fluid.

energy at the surface of separation of fluid and solid to a minimum, which manifests itself in the spontaneous shrinkage of some of these hydrogels, would act so as to raise the vapour pressure with which the gel is in equilibrium, but the operation of this factor would diminish as the surface was diminished by decrease of the water content. These two forces operating simultaneously would alone produce the characteristic gradual diminution in the vapour pressure of the gel as the fluid component is diminished. The break in the direction of the curve

FIG. 2 (reproduced from van Bemmelen, 'Zeits. f. Anorg. Chem.,' vol. 13, 1896, p. 233).—Equilibrium between a Hydrogel of Silica and Water Vapour.



Curve - - - - - the rate of removal of water very slow. In curve ——— much more rapid. The arrows indicate whether the curve shows the removal or the reabsorption of water.

when dehydration has been relatively rapid and is nearly complete, is what must occur when the capillary spaces in the framework become commensurate with the masses (small spheres for instance) of solid out of which the framework is built, and when, therefore, any further diminution in the capillary spaces involves deformation of those masses, unless the removal of water is so slow that the very slow rate of readjustment in the solid phase is not exceeded. Lastly, the very limited powers of reabsorption of fluid by completely dried irreversible gels would, on this view, again not necessarily represent a reformation of the phases, that is to say, a real interaction between silica and water, but the refilling of capillary spaces by water due to the excessive capillary tension of these very minute capillaries. The capacity for reabsorption would therefore be diminished by any agent which facilitates the annealing of the dried gel and so destroys the capillary interspaces. Such an agent is heat, and van Bemmelen found

that brief heating to red heat destroyed the reabsorptive powers of the gel of silica.*

There is one binary system in which gelation is an irreversible process (*i.e.*, coagulation) which can be readily studied under the microscope. The hydrosol is a ternary system composed of water, a minute trace of free acid or alkali, and the modification of egg albumen which is produced by heating it to 100°. Coagulation occurs when the free acid or alkali is removed. As the coagulation point is neared the proteid particles in the hydrosol increase in size, so that spheres 0·75 to 1μ in diameter are formed. These become arranged in rows which anastomose so that an open net with regular polygonal meshes is formed.† In this case the process of gel building is the same as that which can be followed so easily in ternary mixtures, and in both cases a definite surface separates the phases. It is probable that the hydrogel of silica is formed in the same way, since Picton and Linder have shown by optical tests that, as the point of coagulation is approached, larger and larger particles of silica form in the hydrosol.‡ These particles may be solid solutions of water in silica, or they may be large molecular aggregates of silica free from water. I incline to think that the latter is the more probable assumption, since, if they were solutions, it is difficult to see why the process should be irreversible.

In the case of the reversible systems agar-water, or gelatine-water-alcohol, the particles seem to be of the nature of solid solutions.

The system agar and water consists of two components, and, therefore, a nonvariant system should be defined by four coexistent phases. Since the gel stage consists of three phases, namely two solutions and a vapour phase, it should be a monovariant§ system. That is to say, the composition of the phases should be fixed by fixing either the temperature or the pressure of the vapour phase. The experiments show that this is not the case. The composition of the fluid and solid phases is not constant for a given temperature. This result might be regarded as being due to the passive resistance to change in the system which is introduced by the formation of a solid phase. On this view if the velocity of the reaction were known, the phases would be fixed if the element of time were introduced and accorded a definite finite value. This is the method which Bancroft suggests for dealing with such cases ;|| it is, however, possible that there are

* 'Zeits. f. Anorg. Chem.,' vol. 13, 1896, p. 289.

† The process is described in detail in an earlier paper by the author in the 'Journal of Physiology,' vol. 24, 1899, p. 182, and the information which the microscope affords as to the manner in which irreversible gels are built is discussed there.

‡ 'Journal of the Chemical Society,' vol. 61, 1892, p. 148.

§ That is to say, a system having one degree of freedom.

|| 'The Phase Rule,' p. 234.

really more than $n+2$ independent variables, so that the hydrogel is not a monovariant system. In an ordinary system the independent variables are the components (n), temperature and pressure. Agar-water, however, is a system with two components, temperature and two pressures. This follows from the fact that the surface which separates the fluid and solid phases is curved. In point of fact the system is most closely represented by a system of two solutions separated by a membrane which is permeable by only one of the components, for while water will readily pass the surface of separation, agar, having the heavy immobile molecule characteristic of such organic bodies, will be almost unable to do so. Hence, if time be considered finite and small, the surface may practically be considered to be permeable by only one component. As Bancroft* points out, in a system of two solutions separated by semipermeable membrane, there are two pressures and there will be $n+3$ phases in a nonvariant system when n = the number of components. The hydrogel is a system of three phases and, therefore, on this view, to fix the composition, it would be necessary to fix the temperature and one pressure. This relation would probably be true if the curvature of the surface of separation could be fixed. This, however, is not the case, and in order to fix the composition of the phases it would be necessary either to fix the temperature and both pressures, that of the internal as well as of the external phase; or to fix the temperature, one pressure, and the form of the surface. Practically we can only fix the temperature and the pressure of the external phase. I have succeeded in obtaining two phases separated by a plane surface by cooling a hydrosol slowly in an electric field. This method may prove suitable if the system is able to recover from the forces operating during its formation. The method of taking known weights of dry agar and water and keeping them at constant pressure and temperature until equilibrium is obtained is simple, but unfortunately there is the fallacy that the dry agar is a preformed system. The structure of the hydrogel from which it is reproduced is not destroyed by drying, and the system tends to reform itself on the old lines by the filling of the original capillary spaces.

To sum up these remarks, we may describe the hydrogel of agar as a system of three phases, a solid, a fluid, and a vapour phase. The equilibrium is determined by the chemical potential of the components in the various phases, by two pressures, and by temperature. Other operating variables are capillary tension and the energy of the surface between the fluid and solid phases. I have made no measurements to determine how soon the system reaches equilibrium, but the analogous system, gelatine and water, attains to a constant melting point twenty-four hours after the formation of the hydrogel.†

* *Loc. cit.* † 'Gelatinöse Lösungen,' van der Heide, München, 1897.

"A Preliminary Investigation of the Conditions which determine the Stability of Irreversible Hydrosols." By W. B. HARDY, Fellow of Gonville and Caius College, Cambridge. Communicated by F. H. NEVILLE, F.R.S. Received January 12,—Read January 25, 1900.

It has long been held that a large number of colloidal solutions are related to or identical with suspensions of solid matter in a fluid in which the particles of solid are so small as to settle at an infinitely slow rate. Such solutions are the colloidal solutions of metals and of sulphides such as those of antimony, arsenic, and cadmium. Such solutions belong to the class of irreversible colloidal mixtures. A rise of temperature assists the process of coagulation or precipitation;* but neither a further rise nor a fall of temperature will cause the reformation of the hydrosol. On this ground they may provisionally be classed with such colloidal solutions as those of silica, ferric hydrate, alumina, &c., and with the modification of the albumen of white of egg which is produced by heating an aqueous solution to the boiling point. I also add to the class, for reasons to be developed in the following pages, the suspension of mastic in water which is produced by adding a dilute alcoholic solution of the gum to water.

Looked at from the point of view of the phase rule, the equilibrium in these hydrosols, if they really consist of minute solid particles dispersed in a fluid, is not necessarily between the solid particle and water, but between the solid particle and a solution of the particular solid in water. The hydrosol of gum mastic gives off a vapour of the gum of a density sufficient to affect the olfactory organs, and, therefore, the water must contain a definite quantity in solution. Similarly, as it is probable that no substance is completely insoluble, we may assume that in all the examples a portion of the solid is in true solution in the fluid. As the solid which is not in true solution is dispersed in particles whose diameter is, as a rule, very much smaller than the mean wave length of light, it follows that the surface of contact between solid and fluid is very great for unit mass of the former. The opportunity for evaporation and condensation of the solid matter of the particles afforded by the immense surface of contact is so very great that, although only an immeasurably minute quantity of the solid may be in true solution at any one time, this quantity, minute though it be, is probably an important factor in determining the equilibrium between solid and fluid.

* Elsewhere ('Journal of Physiology,' vol. 24, 1899, p. 172) I have shown that precipitation and coagulation are not discontinuous processes. Coagulation gives way to precipitation when the concentration of the solid phase falls below a certain amount.

It is necessary to keep such considerations as these in mind in view of the readiness with which these mixtures have been regarded as simple suspensions* in which the only relation between solid and fluid is a mechanical one. These hydrosols are, as a matter of fact, singularly stable when pure. They can, for instance, be concentrated by boiling to a remarkable extent, and their stability depends upon complex relations between fluid and solid, which gives the former, so to speak, a definite hold over the latter.

Mode of Preparation of the Different Solutions.—The hydrosol of gold was prepared by adding a couple of drops of a solution of phosphorus in ether to about a litre of a very dilute solution of gold chloride. The fine ruby-coloured fluid which was formed was dialysed against distilled water† for fourteen days, and then concentrated by boiling. The hydrosol of silicic acid was prepared by acting on soluble glass with excess of hydrochloric acid, and dialysing the product. A hydrosol of ferric hydrate was prepared by prolonged dialysis of the solution in ferric chloride.

The hydrosol of gum mastic was prepared by adding a very dilute solution of the gum in alcohol to distilled water. It was dialysed for fourteen days against distilled water. The hydrosol of heat-modified egg-white was prepared by dissolving white of egg in nine times its volume of distilled water, filtering and boiling. The result should be a brilliant fluid which scatters blue light. Surface action, however, plays an extraordinary part. If the solution is boiled in a test-tube a milky fluid is formed and a film of proteid is left on the glass; a second quantity boiled in the same test-tube comes out less milky, until, when the proteid film is sufficiently thick to eliminate all action by the glass, the solution after boiling contains the proteid dispersed as particles so small that they scatter pure blue light. After preparation the hydrosol was dialysed against distilled water for some days.

Behaviour of the Hydrosols in an Electric Field.—It has long been known that the particles in these colloidal solutions move in an electric field. Zgismondy‡ found that the gold in colloidal solutions moves against the current. Picton and Linder§ established the important fact that the direction of movement of the particles, as compared with the direction of the current, depends upon their chemical nature. I have shown that the heat-modified proteid is remarkable in that its direction of movement is determined by the reaction acid, or alkaline of the fluid

* *Cf.*, for instance, Stoeckl and Vanino, 'Zeits. f. phys. Chem.,' vol. 30, 1899, p. 98; also Ostwald, 'Lehrbuch.'

† In working with these colloidal solutions it is very necessary to use distilled water freed from dissolved carbonic acid.

‡ 'Lieb. Ann.,' vol. 301, p. 29.

§ 'Journal of Chem. Soc.,' vol. 70, 1897, p. 568.

in which it is suspended.* An immeasurably minute amount of free alkali causes the proteid particles to move against the stream, while in presence of an equally minute amount of free acid the particles move with the stream. In the one case, therefore, the particles are electro-negative, in the other they are electro-positive.

Since one can take a hydrosol in which the particles are electro-negative and, by the addition of free acid, decrease their negativity, and ultimately make them electro-positive, it is clear that there exists some point at which the particles and the fluid in which they are immersed are iso-electric.

This iso-electric point is found to be one of great importance. As it is neared, the stability of the hydrosol diminishes until, at the iso-electric point, it vanishes, and coagulation or precipitation occurs, the one or the other according to whether the concentration of the proteid is high or low, and whether the iso-electric point is reached slowly or quickly, and without or with mechanical agitation.

This conclusion can be verified experimentally in many ways. If a coagulum or precipitate of the proteid particles made either by the addition of a neutral salt, or by the addition of acid or alkalis, be thoroughly washed, made into a fine mud in an agate mortar, and suspended in water in a U-tube, it rapidly subsides. The establishment of an electric field having a potential gradient of 100 volts in 10 cm. has no influence on the level of water or precipitate in forty-eight hours. If, now, the smallest possible amount of caustic soda or acetic acid be added, the proteïd will commence to move, so that in twenty hours the precipitate will rise in one or other limb until it nearly touches the platinum electrode.

Speaking generally, the hydrosol of ferric hydrate is stable only in the absence of free acids or alkalis or neutral salts. The hydrosol of heat-modified proteid is stable only in presence of free acid or alkali. The hydrosol of gum mastic is readily precipitated by acids, but is stable in presence of any concentration of monovalent alkalis. The general conditions of stability of these various hydrosols, therefore, are very different, yet they agree in manifesting the same important relation between the isoelectric point and the point of precipitation as is shown by the hydrosol of proteid.

In the hydrosol of ferric hydrate the particles are markedly electro-positive. A dilute hydrosol is coagulated by citric acid when the concentration of the latter reaches 1 gramme-molecule in 4,000,000 c.c. No matter how small the concentration of the ferric hydrate, the hydrosol becomes cloudy and settles. The rate of settling is, however, slow, being about 1 cm. an hour. In an electric field, having the form of a U-tube, the particles always settle slightly faster from the negative

* "The Coagulation of Proteid by Electricity," W. B. Hardy, 'Journal of Physiology,' vol. 24, 1899, p. 288.

electrode—the acceleration due to the electric field being about 5 mm. an hour. The suspended particles of ferric hydrate show, therefore, an exceedingly slight movement in a direction *opposite* to that which they manifest when in colloidal solution. In the latter condition they are markedly electro-positive; in the former they are exceedingly faintly electro-negative. An exceedingly faint electro-negative character is also conferred upon the ferric hydrate when the hydrosol is coagulated by ammonia, 1 gramme-molecule of the latter being present in 100,000 c.c.

If a fresh gel of silica is broken up in distilled water and carefully washed to free it from still uncoagulated silica, and from impurities, it is completely iso-electric with the water. It becomes markedly electro-negative, however, on the addition of the minutest trace of free alkali.

Gum mastic precipitated from a dilute hydrosol by adding barium chloride until the concentration is 1 gramme-molecule in 600,000 cm. is found to be iso-electric with the fluid. It is markedly electro-negative when in colloidal solution.

Picton and Linder have shown that the particles in these hydrosols gradually grow in size as the coagulation or precipitation point is neared.* It might, therefore, be urged that, as the movement of the particles in the electric field is, on Quincke's theory of electric endosmose, due to surface action, the fact that they do not move when in simple suspension as opposed to colloidal solution may be due to the diminution of the impelling force acting on a given volume.† This is, however, negatived by the character of the experiments. The addition of a minute amount of free alkali to a mass of particles of coagulated silica which have settled to form a "mud" cannot alter the size of these relatively very large masses to any appreciable extent. And since in the case of ferric hydrate and proteid, the sign of the charge which the particles carry in the electric field is different on each side of the actual point of precipitation, that point must of necessity be an iso-electric point.

If the stability of the hydrosol is dependent upon a difference in electrical potential between the solid particles and the fluid, then one would expect that for, at any rate a short distance from the iso-electric point, the stability would vary simultaneously with the variation in the difference of potential. The experimental investigation of this question is beset by many difficulties. At present I know of no way of approaching the iso-electric point other than by the addition of salts,

* 'Journ. of Chem. Soc.,' vol. 61, 1892, p. 148.

† As a matter of fact, Lamb finds that the velocity of a particle is independent of its size or shape, provided that its dimensions are large compared with the slip, so perhaps the objection scarcely needs discussion. Lamb, 'Brit. Assoc. Report,' 1887, p. 502.

acids, or alkalis. One may, therefore, approach the point by the addition of, say, acid or alkali, and use a salt to measure the stability of the system, as in the experiment described later. In such experiments, however, the colloid particles are immersed in a complicated system of three components, the conditions of equilibrium of which cannot be arrived at from existing data. The conditions could be simplified by using, say, KHO or H_2SO_4 to approach the iso-electric point, and K_2SO_4 as a measure of the change of stability. A series of determinations with different systems of this kind may afford the requisite measurements.

A direct and conclusive proof that stability does decrease as the iso-electric point is approached was however obtained in two ways. The iso-electric point can be approached in the case of the hydrosol of proteid by the withdrawal of either the free acid or the free alkali, as the case may be. As it is neared, the proteid particles increase in size, so that instead of scattering blue light, they scatter white light; thus the surface of contact of fluid and solid gradually diminishes as the point is neared. The second experiment, though not a quantitative one, is very convincing. A hydrosol of gum mastic dialysed as pure as possible is not destroyed by mechanical agitation even when long continued. If, however, a salt is added in an amount so small that it just fails to coagulate the hydrosol, the latter is rendered so unstable that it is destroyed by shaking.

Experiments were made to determine whether the particles actually carry a charge. An electric field which was practically uniform was made by using flat electrodes of the same size, which were placed parallel to one another at the ends of a straight tube. The particles were found to move in all parts of the field; they therefore carry a definite charge which, according to Quincke's theory of the movement of particles in an electric field, would be a surface charge, each particle being surrounded by a double layer of electricity.

Action of Salts.—The power possessed by salts of destroying colloidal solutions was noticed by Graham. The subject was, however, first accurately investigated by H. Schulze.* He showed that the power which various salts possess of precipitating a hydrosol of sulphide of arsenic is related to the valency of the metal, while the valency of the acid has little influence. The increase in the precipitating or coagulating power produced by increase in valency is very great. If coagulative power be defined as the inverse of the concentration in gramme-molecules per litre necessary to convert a given hydrosol into a hydrogel, then from Schulze's measurements the coagulative power of metals of different valency is :—

$$R' : R'' : R''' = 1 : 30 : 1650.$$

* 'Journ. f. prakt. Chemie,' vol. 25, 1882, p. 431.

Schulze's conclusions were verified by Prost,* who used sulphide of cadmium, and Picton and Linder, who used the sulphide of antimony.† The last-named workers added the important fact that a small portion of the coagulating salt is decomposed, the metal being entangled in the coagulum.

The measurements which I have made with various colloidal solutions both confirm Schulze's results, and bring out a new relation, which may be stated as follows:—

The coagulative power of a salt is determined by the valency of one of its ions. This prepotent ion is either the negative or the positive ion, according to whether the colloidal particles move down or up the potential gradient. The coagulating ion is always of the opposite electrical sign to the particle.

The salts employed to determine this point were the sulphates of aluminium, copper, magnesium, potassium, and sodium; the chlorides of copper, barium, calcium and sodium, and the nitrate of cadmium. Solutions containing 1 gramme-molecule in 2000 c.c. were prepared.

The experiments may be summarised as follows:—

Silica dialysed free from Chlorides, Electro-negative.

Concentration of Coagulating Salt 1 gramme-mol. in 120,000 c.c. Temperature 16°.

Coagulated at once.	In 10 mins.	In 2 hours.	In 24 hours.	Still fluid.
$Al_2(SO_4)_3$	$CuSO_4$	$MgSO_4$	K_2SO_4	NaCl
	$CuCl_2$		Na_2SO_4	Control.
	$Cd(NO_3)_2$			
	$BaCl_2$			

This illustrates many experiments.

Proteid in presence of trace of Alkali, Electro-negative.

Temperature 16°. Coagulating Salt 1 gramme-mol. in 80,000 c.c.

Coagulated at once.	On slightly warming.	Did not coagulate.
$Al_2(SO_4)_3$	$MgSO_4$	Na_2SO_4
$Cd(NO_3)_2$	$BaCl_2$	K_2SO_4
$CuSO_4$	$CaCl_2$	NaCl
$CuCl_2$		

Proteid in presence of trace of Acetic Acid, Electro-positive.

Coagulated instantly.	No effect.
Al_2SO_4	$CuCl_2$
$CuSO_4$	$Cd(NO_3)_2$
K_2SO_4	$BaCl_2$
Na_2SO_4	NaCl
$MgSO_4$	

* 'Bull. de l'Acad. Roy. de Sci. de Belg.,' ser. 3, vol. 14, 1887, p. 312.

† 'Journ. Chem. Soc.,' vol. 67, 1895, p. 63.

Mastic, Dialysed, Neutral, Electro-negative.

Temperature 16°. Concentration of Coagulating Salts, 1 gramme-mol. in 50,000.

Coagulates at once.	No coagulation.
$Al_2(SO_4)_3$	K_2SO_4
$CuSO_4$	Na_2SO_4
$CuCl_2$	$NaCl$
$Cd(NO_3)_2$	
$MgSO_4$	
$BaCl_2$	

Ferric Hydrate, Dialysed, Neutral, Electro-positive.

Temperature 16°. Coagulating Salt 1 gramme-mol. in 100,000.

Coagulates at once.	Does not coagulate.
$Al_2(SO_4)_3$	$CuCl_2$
$CuSO_4$	$Cd(NO_3)_2$
$MgSO_4$	$NaCl$
K_2SO_4	$BaCl_2$
Na_2SO_4	

Gold, Dialysed for fourteen days against Distilled Water, very faintly Acid. Electro-negative.

Temperature 16°. Coagulating Salt 1 gramme-mol. in 200,000.

Red changes to blue* instantly in—	No change.
$Al_2(SO_4)_3$	$NaCl$
$CuSO_4$	Na_2SO_4
$CuCl_2$	K_2SO_4
$Cd(NO_3)_2$	
$MgSO_4$	
$BaCl_2$	

Only one comment on these experiments is needed. Solutions of $Al_2(SO_4)_3$, $Cd(NO_3)_2$, $CuCl_2$, and $CuSO_4$, are acid to litmus, while $MgSO_4$, and $BaCl_2$ are neutral to litmus, but acid to phenol phthalein. This acidity has a disturbing action in some cases—the system acts not only as a neutral salt, but also as a free acid. Thus the hydrosol of proteid when brought very near to the point of precipitation by dialysis is more sensitive to the more acid than to the less acid salts of the bivalent metals. The effect of the acid or basic reaction of the

* The relation of the colours of hydrosols of gold to the size of the particles has been investigated by Stoeckl and Vanino ('Zeits. f. phys. Chem.,' vol. 30, 1899, p. 98). The change from red to blue indicates an increase in the size of the particles.

salt on the hydrosol is as a rule small compared with the effect of the metal ion. Thus the stability of a hydrosol of electro-positive proteid is increased by free acid, yet the acid salts find their proper place in the scale of valency. Again, ferric hydrate is coagulated by nitric acid when the concentration reaches 1 gramme-mol. in 2500 c.c.; yet the cadmium salt of this acid is not much more potent than the "neutral" salts $MgSO_4$, $BaCl_2$.

Temperature 16°. Concentration necessary to Coagulate Ferric Hydrate.

Salt.			
K_2SO_4	1 gramme-mol. in	4,000,000	c.c.
$MgSO_4$	" "	4,000,000	"
$BaCl_2$.........	" "	10,000	"
NaCl	" "	30,000	"
$Cd(NO_3)_2$ ...	" "	50,000	"

The extraordinary rise in coagulative power with an increase in valency, which was observed by Schulze, Prost, and Picton and Linder, holds in all cases. In order to measure it for ferric hydrate, I used Schulze's method, in which a drop of the hydrosol is allowed to fall into a large volume of the solution of the salt. A number of experiments were made until the concentration of the salt was found which just sufficed to coagulate the drop. In the case of gold and mastic the process was reversed, the salt solution being added drop by drop to a measured quantity of the hydrosol. I append the results:—

Gum Mastic, Neutral. Temperature 40°.

$BaCl_2$	1 gramme-mol. in	86,000	c.c.
NaCl	" "	8,000	"
$MgSO_4$	" "	68,000	"
K_2SO_4............	" "	8,000	"

Gold, very faintly acid. Temperature 16°.

NaCl	1 gramme-mol. in	72,000	c.c.
$BaCl_2$	" "	500,000	"
K_2SO_4	" "	75,000	"

The figures for ferric hydrate have already been given. It has been pointed out that if specific molecular coagulative power be defined as the inverse of the volume occupied by one gramme-molecule of a substance when it just suffices to bring about coagulation, then this value (K) varies with the valency of the active ion approximately according to the square and cube:—

$$R' : R'' : R''' = K : K^2 : K^3.$$

The relation really is not as simple as this; it is complicated by the change which the specific molecular conductivity of a salt undergoes with change in concentration. The theoretical considerations have been dealt with elsewhere.* For convenience of description, however, I will call this relation the relation of the square and cube.

Action of Acids and Alkalis.—The values for K furnished by these substances show relations to valency even more interesting than that found with salts. As in the case of salts, their action is entirely dependent upon the electric properties of the colloid particles.

When the colloid particles are electro-negative, alkalis either do not cause precipitation at any concentration, or if they do cause precipitation the value of K does not vary in any simple way with variations in valency.

When the particles are electro-positive, K increases with valency, but the relation of the square and cube does not hold. Instead, one finds that K varies directly with the chemical activity of the solution.

Acids have the reverse relations. When the particles are electro-negative, the value of K varies directly with the chemical activity of the solution; while if these particles are electro-positive, acids either have no precipitating power, or if K has any value, then (in the particular case measured) the value varies with valency according to the square and cube.

The various measurements are brought together in the following table. The specific conductivities were calculated from the British Association tables.†

* Hardy and Whetham, 'Journal of Physiology,' vol. 24, 1899, p. 288, and Whetham, 'Phil. Mag.,' November, 1899.

† "The Electro-chemical Properties of Aqueous Solutions." T. C. Fitzpatrick 'Brit. Assoc. Report,' 1893.

Coagulating Solution.

Hydrosol.		Concentration necessary to produce coagulation. 1 gramme-mol. in c.c. =	Gramme equiv. per litre.	Specific conductivity of coagulating solution at 18° × 10^{13}.	Temperature.	
Mastic, electro-negative.	Ammonia...	0	∞	..	16—100°	
	KaOH	0	∞	..	16—100	
	KOH	0	∞	..	16—100	
	$Ba(OH)_2$...	40,800	0·048	100	16	
	H_3PO_4.....	194,400	0·015	13·9	17	
	Acetic acid .	1,360	0·7	12·6	17	
	HCl	260,000	0·004	14·5	17	
	HNO_3......	260,000	0·004	14·3	17	
	H_2SO_4	460,000	0·004	13·2	17	
	Oxalic acid..	220,000	0·009	14·4	17	
	$BaCl_2$......	86,000	0·022	[20]	40	The values for specific conductivity are given for a temperature different from that of observation, but they serve to show the general relation.
	$MgSO_4$.....	68,000	0·028	[18]	40	
	NaCl	8,000	0·12	[110]	40	
	K_2SO_4......	8,000	0·24	[250]	40	
Gold, electro-negative.	Ammonia...	0	∞	..	17—100	
	NaOH	11,800	0·08	152	17	
	KOH	10,800	0·09	189	17	
	$Ba(OH)_2$...	12,000	0·16	..	100	Solution saturated at 17° has no action.

Coagulating Solution—*continued.*

Hydrosol.		Concentration necessary to produce coagulation.		Specific conductivity of coagulating solution at 18° × 10[12].	Temperature.	
		1 gramme-mol. in c.c. =	Gramme equiv. per litre.			
Ferric hydrate, electro-positive.	$Ca(OH)_2$ ...	..	..	..	17°	No action when saturated.
	HCl	123,000	0·008	29	17	
	H_2SO_4	238,544	0·0084	26	17	
	NaC	72,000	0·013	13	17	
	$BaCl_2$	500,000	0·004	4·4	17	
	K_2SO_4......	75,000	0·026	28	17	
	KHO	1,000,000	0·001	2·2	16	
	$Ba(OH)_2$...	2,000,000	0·001	2·3	16	
	HCl	1,800	0·5	1650	16	
	HNO_3	2 000	0·5	1589	16	
	H_2SO_4	1,000,000	0·002	6·8	16	
	Oxalic acid .	1,000,000	0·002	3·4	16	
	Citric „ .	4,000,000	0·0007	[0·7]	16	Specific conductivity by analogy with similar acids will not be greater than the value given.
	K_2SO_4	3,200,000	0·0006	0·77	16	
	$MgSO_4$.....	4,000,000	0·0005	0·5	16	
	$BaCl_2$......	6,000	0·3	255	16	
	NaCl	20,000	0·5	28	16	

The figures in the fourth column are very remarkable. When the particles are electro-negative, equicoagulative solutions of acids agree in their electric conductivity within the limits of experimental error. The same relation is clearly shown if one takes the measurements which Picton and Linder made of the power possessed by acids of coagulating the hydrosol of arsenious sulphide.

Acid.	Value of K referred to Al_2Cl_6 as unity.	Sp. mol. conductivity when 1 gr. equiv. = 1000 c.c.
HBr, HI, HCl, HNO_3	0·001	2950
H_2SO_4	0·0006	1935
Oxalic	0·0005	578
H_3PO_4	0·00007	230

When, however, the particles are electro-positive, the conductivity of equicoagulative strengths of acids varies to a remarkable extent.

Acids.	H′	:	H″	:	H‴
Mastic, electro-negative	12·6		14·4		13·9
Ferric hydrate, electro-positive	1650		6·8		0·7

Now specific conductivity (C) has the relation

$$C = n\alpha(u+v)$$

where α is the fraction of the total number of molecules (n) which are dissociated at any one moment, and $u + v$ is the sum of the velocities of the two ions. The factor $u + v$ plays an important part, as will be seen by comparing the values for $n\alpha$ in equicoagulative solutions of acids with slowly moving ions with those with rapidly moving ions :—

	$n\alpha$.
H_3PO_4	0·01
Acetic acid	0·07
HCl, H_2SO_4, HNO_3	0·004

This, however, is probably partly due to the fact that owing to the manner in which the coagulative power was measured, time has practically a constant small value. The values for n might, perhaps, be different if the duration of the experiments were prolonged indefinitely.

The important point, however, which calls for notice is that the function $\alpha(u + v)$ is a numerical measure of the chemical activity of the substance at a given concentration, so that we reach the important conclusion that *the concentration of acids necessary to coagulate electro-negative colloid particles, and of alkalis necessary to coagulate electro-positive particles, is determined by the laws which govern ordinary chemical equilibrium.*

In the case of the action of salts on these hydrosols, the relation is not so simple. K does not vary directly with $\alpha(u + v)$, but contains a factor which is approximately squared or cubed by a change from a mono-valent to di- or tri-valent ions. The relation can therefore be best expressed as

$$K = n\alpha(u+v)A^x$$

where x is positive and increases rapidly with an increase in the valency of the ion whose electric charge is of the opposite sign to that on the particles.

I should interpret these relations by the suggestion that in the former the acid or alkali alters the difference of potential at the surface of the particles by altering the character of the fluid, and in that way modifies the stability of the hydrosol; in the latter the active ions of the salt act directly upon the solid particles, or, perhaps, on the charge which these carry, and thus play a part which is, perhaps, generally similar to the action of ions when they furnish nuclei for the condensation of vapour. Picton and Linder have shown that the active ions are actually entangled in, and form part of, the coagulum.*

The former relation may profitably be placed beside Brühl's conclusions that the action exerted by a fluid upon the substance dissolved in it is determined by the chemical characters of the former, as well as of the latter. He has shown that the molecular refraction, the dielectric coefficient, and the power possessed by the fluid of dissociating or chemically changing the molecules of the substance dissolved in it are measured by the unsatisfied valency, or, to use another phrase, the residual energy of its molecules.

The action of acids or alkalis on a hydrosol, the particles of which are of the opposite electrical sign, seems to be compounded of these two actions. The acid or alkali may act as a salt, and exhibit the characteristic relation between K and the valency of the ion of the opposite electrical sign. An instance is furnished by the action of various acids on ferric hydrate, or the acid or alkali by *increasing* the difference of potential between the fluid and the solid particles may increase the stability of the hydrosol. This is markedly manifested by the increased stability given to the hydrosol of gum mastic by the addition of univalent alkalis. In the action of barium hydrate on this hydrosol, the segregating action of the metal ion overcomes the

* 'Journ. of the Chem. Soc.,' vol. 67, 1895, p. 63.

action exerted by the reagent in virtue of its alkalinity, the result is that the coagulative concentration of the alkali $Ba(OH)_2$ gives a value for K which is less than that given by salts of bivalent metals, and the specific conductivity of the solution is of the same order as that of the coagulating concentration of salts of univalent metals. Against these suggestions, however, must be set the anomalous relations of the various alkalis to the hydrosol of gold.

Action of a Salt in presence of Varying Amounts of Acid or Alkali.

This was measured for one salt only, potassium sulphate, the colloidal solution being gold. The figures are as follows :—

Temperature 16°

	Concentration 1 gr.-mol. in— c.c.	Concentration of the salt necessary to produce blue tint. 1 gr.-mol. K_2SO_4 in— c.c.
Acetic acid.........	1,087	0
	16,000	324,000
	66,000	64,000
	330,000	50,000
	(neutral*)	28,500
Ammonia	113,333	10,000
	22,666	9,000
	4,900	20,000
	2,450	24,000
	980	20,000
	200	Large amount of salt needed.
	100	Salt unable to act when saturated at 16° or at 100°.

These results are shown in the curve. Ammonia alone will not aggregate the particles of gold. Up to a certain point, however, it decreases the stability of the system.

The conclusions can be summarised as follows :—The irreversible hydrosols which have been investigated are systems composed of solid particles dispersed through a solution of the substance of the solids in the water.

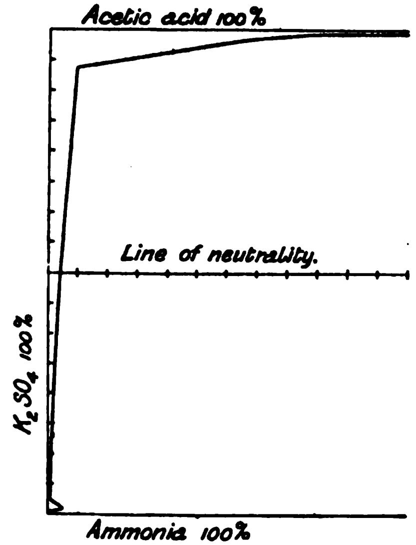
The stability of the system is related to the contact difference of potential which exists between the solid and the fluid phases, and which forms round each solid particle a double electrical layer. Such double electric layers round particles of any kind immersed in a fluid

* Except for a faint acid reaction of the gold solution, due probably to a trace of phosphoric acid.

would resist any movement of the particles through the fluid, because, as Dorn's experiments show, electric work is done in displacing the particles.* The effect would be the same as if the viscosity of the fluid was increased.†

The stability of the system may be destroyed by altering the difference of potential. Free acid, added to a hydrosol in which the particles are negative to pure water, will diminish the relative differ-

FIG. 1.



Action of potassium sulphate in presence of varying amounts of acetic acid or ammonia upon the hydrosol of gold. The abscissæ represent the volume of water which holds 1 gramme-molecule of the salt. The positive ordinates represent the reciprocals of the volume which holds 1 gramme-molecule of the acid, and the negative ordinates the reciprocals of the volume which holds 1 gramme-molecule of the alkali. Each division = 50,000 c.c.

ence of potential of the water. In this case the reagent acts directly on the water, and the coagulative activity of unit mass of the substance varies directly with its chemical activity when dissolved in water. The same relation seems to hold when free alkali is added to a hydrosol in which the particles are electro-positive.

The stability of the system may also be destroyed by induction, the active agents being free ions carrying a static charge.‡ In this case

'Wied. Ann.,' vol. 10, 1880, p. 70.

† This mode of stating the result I owe to Professor J. J. Thomson, and gladly acknowledge his kindness in discussing this and kindred points with me.

Whetham, *loc. cit.*

the action may be said to be on the particles, or rather on the electric layers immediately around them, and the active ions are those whose electric sign is the opposite of that of the charge on the surface of the particles. In this case coagulative power does not vary directly with variations in chemical activity. It rises exceedingly rapidly with a rise in the valency of the active ion, so that the relation

$$I' : I'' : I''' = n : n^2 : n^3$$

is approximately satisfied.

Picton and Linder have shown that when the concentration of the salt is insufficient to completely destroy the system, it is not wholly without action. A fresh point of equilibrium between solid and fluid is reached by an increase in the size of the particles and therefore a diminution in the extent and curvature of the surface of contact. The fact is of importance, because it introduces us to the possibility that the reagent may affect the size of the particles by altering the equilibrium between the part of the solid in solution and the part in suspension. Double electric layers round each particle are, according to Thomson, separated by a region of "uncompleted chemical combination" between the components.* The density of the field round the particles in hydrosols will therefore be a measure of the velocity of the solution and condensation between the particle and the liquid, and therefore the factor which determines whether the particles will on the whole grow, diminish, or remain stationary in size.

When acids or alkalis are added to hydrosols holding particles of the opposite electric sign to themselves, the simplest relation seems to be that univalent acids or alkalis increase the stability; bivalent acids or alkalis decrease it.

The view advanced in this paper implies that each particle in a hydrosol is surrounded by a zone in which the components are in a condition of chemical instability. According to Rayleigh,† such a zone is of finite thickness, and deep enough to contain several molecules. We therefore have in these hydrosols two phases, separated by a layer of extraordinarily large extent, which possesses considerable chemical energy. This, it seems to me, suggests an explanation of the catalytic powers so markedly manifested by hydrosols.

* 'Discharge of Electricity through Gases.' Scribner, 1898, p. 24.

† Thomson, *loc. cit.*, p. 26; Rayleigh, 'Phil. Mag.,' vol. 33, 1892, p. 468.

"The Piscian Stars." By Sir NORMAN LOCKYER, K.C.B., F.R.S.

Received November 20,—Read December 14, 1899.

Introductory.

In the Bakerian Lecture for 1888* I briefly considered the relation of the stars having spectra with predominant carbon absorption (for which I have recently suggested the name *Piscian*†) to the various other types of celestial bodies. Shortly afterwards I began the discussion of the observations which had been made by Dunér in the case of these stars. Those relating to Group II (now named *Antarian*), another group studied by Dunér, were fully dealt with in the Bakerian Lecture, but after all the available information as to the former had been brought together, I found that notwithstanding the admirable data which Dunér had put on record, there were some points on which further information was desirable.

In the Antarian stars we had evidence as to the lines present in company with the various flutings, but in the Piscian stars even the presence or absence of lines was somewhat uncertain.

The publication of this investigation was therefore postponed to see what light could be thrown upon the subject by further observations. At various times, as the work permitted, such observations have been attempted, and the results, so far as they went, did not disturb the classification at which I had already arrived. Still, the information thus gained was less complete than desired.

The photographic work which has quite recently been done on these stars by Dr. McClean and Professor Hale has now furnished the additional information required, and it is therefore unnecessary to delay the publication of the memoir, some ten years old, which in the main points stands as it was written.

Historical Statement.

Secchi was the first to recognise stars with spectra of the type under discussion during his spectroscopic survey made in 1866 and 1867. They constituted his fourth type. All the stars of the group are of small magnitude, and of a deep red colour. He was inclined to believe that a radiation spectrum was in question, but pointed out that there was a relation between the flutings of carbon and the dark bands seen in the spectra of the stars. He says:—" Quelques-unes des raies noires, et les plus importantes, coincident à très-peu près avec celles du troisième type; cependant le spectre, dans son ensemble, se présente

* 'Roy. Soc. Proc.,' vol. 44, p. 26.

† 'Roy. Soc. Proc.' (1899), vol. 65, p. 191.

comme un spectre direct appartenant à un corps gazeux, plutôt que comme un spectre d'absorption. Si on le considère comme un spectre d'absorption on trouve qu'il présente le caractère des composés du carbone, tels qu'on les obtient en produisant une série d'étincelles électriques dans un mélange de vapeur de benzine et d'air atmosphérique et dans l'arc voltaique entres les charbons."*

Secchi also states† that bright lines are occasionally seen in the spectra of stars of this type, and in a diagram of the spectrum of 152 Schjellerup he indicates no less than six bright lines.

Dunér in 1884 catalogued fifty-five stars of this group, and recorded the details of their spectra, so far as he was able to observe them.‡

Referring to Secchi's work, Dunér says:—"Secchi s'est beaucoup occupé de ces spectres, mais il y a de très graves erreurs dans ce qu'il dit sur l'apparence qu'ils offrent et sur leur nature. D'abord il prononce à plusieurs occasions dans sa 'Memoria seconda' et dans son ouvrage 'Le Soleil' que le rouge leur manque presque absolument. Pour ma part, je l'ai trouvé très vif dans tous, seulement un peu pâle en comparaison avec la sous-zone jaune excessivement brillante. Puis Secchi parle des raies *vives* qui termineraient, du côté du violet, les zones brillantes. Mais ni dans la zone verte ni dans la bleu je n'ai vu la moindre chose qui pût expliquer un tel énoncé, et je sais que M. Vogel n'a pas été plus heureux. Quant aux deux raies brillantes que Secchi dit avoir vu dans le jaune, elles se rapportent selon toute probabilité à la sous-zone jaune, laquelle, comme je viens de le dire, est divisée en deux par une bande étroite. Secchi s'est plus tard persuadé, par des mesures, que les deux raies jaunes n'ont pas la même position que celles du sodium ; mais il est néanmoins difficile de comprendre comment il a pu croire que cette zone, quarante fois plus large que la distance entre D_1 et D_2, fût les raies du sodium. Au reste il paraît disposé à admettre que tout le spectre est un spectre direct émis par un gaz incandescent. Pour moi, il est tout à fait incontestable que c'est un spectre d'absorption, tout aussi bien que celui des étoiles de la Classe IIIa, et M. Vogel a déjà, il y a quelques années, emis une pareille opinion, et il l'a répétée tout récemment."§

In 1865 Zöllner pointed out that spectra might enable us to determine the relative ages of celestial bodies, and suggested that the yellow and red light of certain stars were indications of a reduction of temperature.‖ There is now no doubt that stars with fluted spectra, whether of Group II (Antarian) or Group VI (Piscian), are cooler than stars like the sun and α Lyræ, which have line spectra. But there is an

* 'Le Soleil,' vol. 2, p. 458.
† *Ibid.*, p. 457.
‡ 'Sur les Étoiles à Spectre de la Troisième Classe,' Stockholm, 1884.
§ *Ibid.*, p. 10.
‖ 'Phot. Unters.,' p. 243.

important difference between the two groups, as I pointed out in the Bakerian Lecture. In the Antarian stars we have to deal with condensing swarms of meteorites in which the temperature is increasing, whereas in the Piscian stars we have an advanced stage in the cooling of masses of meteoritic vapours. In the case of Antarian stars we have mixed radiation and absorption flutings, and I suggested a way in which the stars of the group might be divided into fifteen distinct species, representing successive stages in condensation. We have now to consider a similar classification of the Piscian stars, of which I stated in the Bakerian Lecture (p. 26):—"The species of which it will ultimately be composed are already apparently shadowed forth in the map which accompanies Dunér's volume, and they will evidently be subsequently differentiated by the gradual addition of other absorptions to that of carbon, while at the same time the absorption of carbon gets less and less distinct."

In considering the stars of this group, it is most important to bear in mind that there are indications of carbon absorption in the spectrum of the sun. I first obtained evidence of the existence of carbon vapour in the solar atmosphere in 1874, and in 1878 I communicated a paper to the Royal Society on that subject.* Ångström had already shown that the true carbon *lines* were not reversed in the solar spectrum, but I demonstrated by photographic comparisons that there was a perfect correspondence between the individual members of the brightest part of the fluting in the ultra violet (commencing at λ 3883·55), and a series of fine dark lines in the solar spectrum. I pointed out that this carbon vapour existed in a more complicated molecular condition (as is evidenced by the flutings) than the metallic vapours in the sun's atmosphere.†

There can therefore be no doubt that in the sun carbon absorption is just commencing, and that, as I stated in a former paper,‡ "the indications of carbon will go on increasing in intensity slowly, until a stage is reached when, owing to reduction of temperature of the most effective absorbing layer, the chief absorption will be that of carbon—a stage in which we now find the stars of Class III*b* of Vogel's classification."

* 'Roy. Soc. Proc.,' vol. 27, p. 308.

† *Note* (*added* 1899).—Professor Rowland has since identified a considerable number of faint lines in the solar spectrum, on the more refrangible side of *b*, with the constituents of the green carbon fluting ('Prelim. Table of Solar Spectrum Wave-lengths,' p. 90), and we thus have direct evidence of the presence in the solar spectrum of the band which is perhaps the most characteristic feature of the Piscian stars.

‡ 'Roy. Soc. Proc.,' vol. 43 (1887), p. 155.

General Characteristics of the Spectra.

The main features of the spectra are three broad dark flutings, which fade off to the violet end of the spectrum. These, as is now well known, coincide with the three principal bands in the spectrum of high temperature* carbon.

The wave-lengths of the bands, and Dunér's numbers, are as follows :—

No. of band.	Duner's mean λ.	Vogel's mean λ.†	Hot carbon (Ångström).
6	563·3	563·1	563·3
9	516·3	515·9	516·4
10	472·7	472·9	473·6

The greatest discrepancy is in the case of band 10, and this is easily explained when we consider the variation of the position of the maximum intensity of the band to which I have previously drawn attention.‡ At different temperatures the position of the brightest part of the band changes, and in Ångström's measure of the carbon fluting this was not taken into account.

In addition to these principal bands, Dunér mapped seven secondary bands. In the Bakerian Lecture, I stated that "there is evidence that some of the absorption is produced by substances which remain in the atmosphere during the next stage, that of Group VII (dark bodies). This probability is based upon the fact that some of the bands are apparently coincident with bands in the telluric spectrum as mapped by Brewster, Ångström, Smyth, and others."§

* As in former papers, the term "high temperature," as used here, is only relative, and refers to the spectrum of carbon which is seen in the electric arc, Bunsen burner, or vacuum tube under certain conditions. The spectrum of carbon at a still higher temperature consists of lines.

† 'Potsdam Observations,' No. 14, 1884, p. 31.

‡ 'Roy. Soc. Proc.,' vol. 45 (1889), p. 167.

§ *Note* (*added* 1899).—Although my subdivision of the group into species, made ten years ago, is based in part upon the secondary bands, it is not materially affected by the new information as to the nature of these bands, since, as cooling goes on, low temperature metallic lines would become more prominent relatively, just as we might have expected the secondary bands to become stronger on the supposition that they had the same origin as the telluric bands.

Specific differences in the Spectra.

In considering the question of variations of spectra with temperature in these stars, the importance of taking differences of magnitude into account must not be lost sight of.

A general examination of Dunér's observations indicates that there are two marked differences in the spectra of the different stars. (1) Some of them give secondary bands, whilst in others they are absent. (2) Some of them have longer continuous spectra than others, as indicated by the number of "zones" visible.

If the continuous spectrum extends far enough towards the violet, the three dark flutings of carbon will divide the spectrum into four bright zones. If it does not extend beyond the most refrangible of the flutings (λ473) only three zones will be visible, and the continuous spectrum will appear to end sharply at wave-length 473. In one case it does not extend beyond the fluting at λ517, and then only two zones are visible.

These differences might evidently depend upon differences of magnitude of the stars concerned, but a detailed examination of the observations shows that some of the differences do not depend upon brightness. If we consider the visibility of the secondary bands according to Dunér, we have the following result:—

No. of band.	Wave-length.	Magnitude of stars in which it is seen.	Magnitude of stars in which it is not seen.
1	656·0	5·4—6·2	6·0—9·5
2	621·0	5·4—8·1	6·0—9·5
3	604·8	5·4—8·1	6·0—9·5
4	589·8	5·4—8·5	6·6—9·5
5	576·0	5·4—8·1	7·5—9·5
7	551·0	5·4—6·5	6·0—9·5
8	528·3	5·4—7·0	6·0—9·5

This table shows that the visibility of the secondary bands is not altogether dependent upon the magnitudes of the stars observed. Thus the bands 2 and 3 are seen in some of the stars of the group as low as magnitude 8·1, whilst they are absent from some of the stars of the sixth magnitude.

If we consider the question of the length of the continuous spectrum, we have the following result, the maximum number of zones referring to the longest continuous spectrum:—

Number of zones.	Magnitudes of stars in which they are seen.
4	5·5—8·2
3	6·0—9·5
2	8·0—9·0

Here, again, the visibility of the zones does not depend altogether upon thé magnitude. There are stars as bright as the sixth magnitude which only give three zones, whilst some as low as 8·2 give four. Dunér refers to this difference as follows:—

"Puis l'intensité de la lumière des zones brillantes peut varier considérablement chez les étoiles de la même grandeur. Dans les étoiles d'un rouge foncé, la zone ultra-bleue est extrêmement faible en comparaison avec la même zone dans les étoiles rouge jaune; et chez les étoiles faibles, cette zone est tout-à-fait invisible, et même la zone bleue est très difficile à voir si elles sont très rouge."*

Another important difference is the variation in the intensity of the citron band of carbon (band 6) as compared with the other bands. Dunér also refers to this point (p. 10) as follows:—"Mais aussi la bande principale à la longueur d'onde 563 est d'une opacité très variée. Chez certaines étoiles, elle est presque aussi foncée que les deux autres bandes principales; mais dans certains spectres elle est assez faible, et semble, probablement à cause de cela, être beaucoup moins large que les bandes aux longueurs d'onde 516 et 473. Celles-ci, et surtout la première d'entre elles, sont toujours très fortes et très larges, et forment le caractère le plus prononcé de ces spectres." A discussion of the observations shows that this variation is independent of the magnitudes of the stars.

Thus we find that the band is dark in some stars with magnitudes varying from 5·4 to 8·0. It is interesting to note that this band, as I have shown, is also the one most subject to variations in the spectra of comets.†

It thus appears that there are distinct differences in the spectra, quite independent of the difficulties in recording the details.

It will be clear that the stars with the longest spectra, characterised by four "zones," must be placed above those with shorter spectra on the temperature curve. As none of the stars, however, show more than four zones and only one less than three, this alone does not discriminate sufficiently between the different species, and we have therefore to look to the variations in the carbon flutings and secondary bands for finer sub-division.

* 'Sur les Étoiles,' &c., p. 9.

† 'Roy. Soc. Proc.,' vol. 45 (1889), p. 168.

In the last stages of all, before final extinction, the stars will be so feeble that the details of their spectra cannot be recorded; so that the expected phenomena, on following out continuity of changes, cannot be checked by direct observation. The sequence of changes which we should expect to occur would be :—

(1) The gradual darkening of the carbon flutings and subsequent paling as the continuous spectrum becomes weaker.

(2) The gradual fading out of those solar lines which do not persist as low as flame temperature.

(3) The gradual increase in the intensity of absorption lines or bands representative of low temperature.

Taking Dunér's observations as we find them, we arrange the stars in seven species, particulars of which follow :—

Species 1.

Characteristics.—Four zones. Band 6 pale; secondary bands 4 and 5 present, or perhaps 4 without 5.

No. 3, 7 *Schj.*, *Mag.* 7·0 *Rrj.*—Four zones, band 6 weaker than usual; band 4 fairly distinct.

No. 18, 74 *Schj.*, *Mag.* 6·5 *Rrj.*—Four zones; band 6 rather weak; 4 and 5 well seen.

Species 2.

Characteristics—Four zones. Band 6 pale; secondary bands 2, 3, 4, and 5 present.

No. 11, 51 *Schj.*, *Mag.* 6·3 *Rrj.*—Four zones. Band 6 weaker than 9 and 10; 2, 3, 4, and 5 seen.

Species 3.

Characteristics—Four zones. Band 6 pale; secondary bands 1, 2, 3, 4, 5, 7, and 8.

No. 25, 132 *Schj.* (*U Hydræ*), *Mag.* 5·4 (*Var.*) *Rrj.*—Four zones. Band 6 weak; 1, 2, 3, 4, 5, 7, and 8 strongly developed.

No. 26, *D.M.* + 68° 617, *Mag.* 6·2 *Rrj.*—Four zones. Band 6 weaker than 9 and 10; 1, 2, 3 rather weak; 4 and 5 well seen; 7 and 8 seen with difficulty.

No. 55, 19 *Piscium*, *Mag.* 6·2 *Rrj.*—Four zones. Band 6 relatively feeble; 1, 2, 3, 4, 5, 7, and 8 are visible.

Species 4.

Characteristics.—Four zones. Band 6 dark; secondary bands all visible.

No. 29, 152 *Schj.*, *Mag.* 5·5 *Rrj.*—Four zones. Carbon bands wide and dark; secondary bands 3, 4, 5 well seen; 1, 2, 7, and 8 feebly visible.

No. 40, 229 *Schj.*, *Mag.* 6·5 *Rrj.*—Four zones. Carbon bands wide and dark; bands 2, 3, 4, 5, and 8 seen, but weak.

No. 52, 249*a* *Schj.*, *Mag.* 6·2 *Rrj.*—Four zones. Bands 2, 3, 4, 5, 7, and 8 well seen.

Species 5.

Characteristics.—Three zones, with trace of ultra-blue zone. Carbon band dark. All secondary bands visible in the brighter stars. In the fainter stars bands 1, 2, and 3 would probably not be seen, as they are in an obscure part of the spectrum.

No. 6, *D.M.* + 57° 702, *Mag.* 7·9 *Rrj.*—Four zones; ultra-blue excessively weak. Carbon bands wide and dark; 4 and 5 well seen; 7 and 8 possibly visible.

Species 6.

Characteristics.—Three zones. Band 6 dark. These stars will in general be dim, so that the secondary bands will only be well seen in the brighter stars of the species.

No. 1, 3 *Schj.*, *Mag.* 8·2 *Rrrj.*—Three zones; blue very pale. Band 6 wide and dark. Band 4 probably visible.

No. 2, *D.M.* + 34° 56, *Mag.* 8·1 *Rrrj.*—Three zones. Band 4 glimpsed.

No. 7, 27*a* *Schj.*, *Mag.* 6·6 *Rrj.*—Three zones; blue faint. Band 2 probably seen.

No. 8, 41 *Schj.*, *Mag.* 7·0 *Rrj.*—Three zones. Carbon bands wide and dark; bands 4 and 5 seen, and band 8 glimpsed.

No. 13, *S Aurigæ* (*Var.*) *Rrrj.*—Two zones only. Spectrum very weak, but band 6 is dark. Very little detail was observed here, probably on account of the faintness of the spectrum, the star being only magnitude 9·4 at maximum.

No. 15, 64*a* *Schj.*, *Mag.* 7·7 *Rrj.*—Three zones; blue weak. Carbon bands very strong; bands 4 and 5 well seen; 2 and 3 glimpsed.

No. 21, 89 *Schj.*, *Mag.* 7·5 *Rrj.*—Three zones. Band 6 rather dark.

No. 24, 124 *Schj.*, *Mag.* 6·5 *Rrj.*—Three zones. Carbon bands very wide and dark; 4 and 5 well seen.

No. 27, 136 *Schj.*, *Mag.* 6·0 *Rrrj.*—Three zones. Bands 4 and 5 well seen.

No. 28, 145 *Schj.*, *Mag.* 8·1 *Rrj.*—Three zones. Bands 4 and 5 well seen; 2 and 3 possibly present.

No. 30, 155*b* *Schj.*, *Mag.* 7·3 *Rrj.*—Three zones. Carbon bands very dark; bands 4 and 5 well seen; 1, 2, 3 very feebly visible.

No. 33, 202 *Schj.*, *Mag.* 8·5 *Rrrj.*—Three zones. Band 6 dark; ? band 4.

No. 35, *D.M.* + 36° 3168, *Mag.* 8·5 *Rrj.*—Three zones. Carbon bands wide and dark; trace of band 4.

No. 37, 219 *Schj.*, *Mag.* 8·0 *Rrj.*—Three zones. Carbon bands very wide and dark; bands 4 and 5 seen; band 2 present, but very weak.

No. 38, 222 *Schj.*, *Mag.* 9·0 *Rrrj.*—Three zones; blue very weak. Carbon bands strong. No secondary bands were recorded, probably because of the faintness of the spectrum.

No. 39, 222*e* *Schj.*, *Mag.* 7·3 *Rrrj.*—Three zones; blue very weak; 4 and 5 distinctly visible.

No. 41, 228 *Schj.*, *Mag.* 7·0 *Rj.*—Three zones. Bands 2, 3, 4, and 5 visible.

No. 42, *D.M.* + 32° 3522, *Mag.* 8·0 *Rrj.*—Three zones; blue rather bright. Carbon bands very wide and dark; 4 and 5 seen.

No. 45, *D.M.* + 35° 4002, *Mag.* 9·5 *Rrj.*—Three zones. Carbon bands very wide and dark. (The absence of secondary bands probably due to faintness of star.)

No. 49, *V Cygni* (*Var.*) *Rrrj.*—Three zones. Band 6 wide and dark.

No. 51, *S Cephei* (*Var.*) *Rrrj.*—Three zones. Band 6 wide and dark.

No. 53, 251 *Schj.*, *Mag.* 7·8 *Rrrj.*—Three zones. Bands 4 and 5 doubtful.

Species 7.

Characteristics.—Three zones. Band 6 pale.

No. 9, 43 *Schj.*, *Mag.* 8·1 *Rrrj.*—Three zones. Band 9 strong; band 6 pale.

No. 12, 99 *Birm.*, *Mag.* 8·0 *Rrj.*—Three zones. Band 9 very strong; band 6 weak.

No. 16, 72 *Schj.*, *Mag.* 7·4 *Rrj.*—Three zones. Bands 9 and 10 wide and dark; band 6 weaker; possibly bands 4 and 5 are visible.

No. 31, *V Coronæ* (*Var.*) *Rrj.*—Three zones. Band 9 strong; 6 rather weak.

No. 48, *U Cygni* (*Var.*) *Rrrj.*—Three feeble zones. Band 6 weak.

The remaining stars of the group observed by Dunér are not described with sufficient detail to enable them to be included in the foregoing classification.

The variations from one species to another are shown in the accompanying map (p. 136), which also indicates the connection with stars of the group which precedes it, and suggests a later Species 8, not yet identified by observation.

Owing to the difficulties attending the observations, the actual appearances of the secondary bands are not quite so regular as is shown in the map.

The Colours of the Piscian Stars.

The classification arrived at was next tested by reference to the colour phenomena. Dunér employs two methods of indicating the colours of the stars, which he has specially observed—first, by means of initial letters, and second, by means of numbers, such that 10 = blood red. Thus:

Rj = rouge jaune.
Rrj = rouge jaune foncé.
Rrrj = presque rouge absolue.

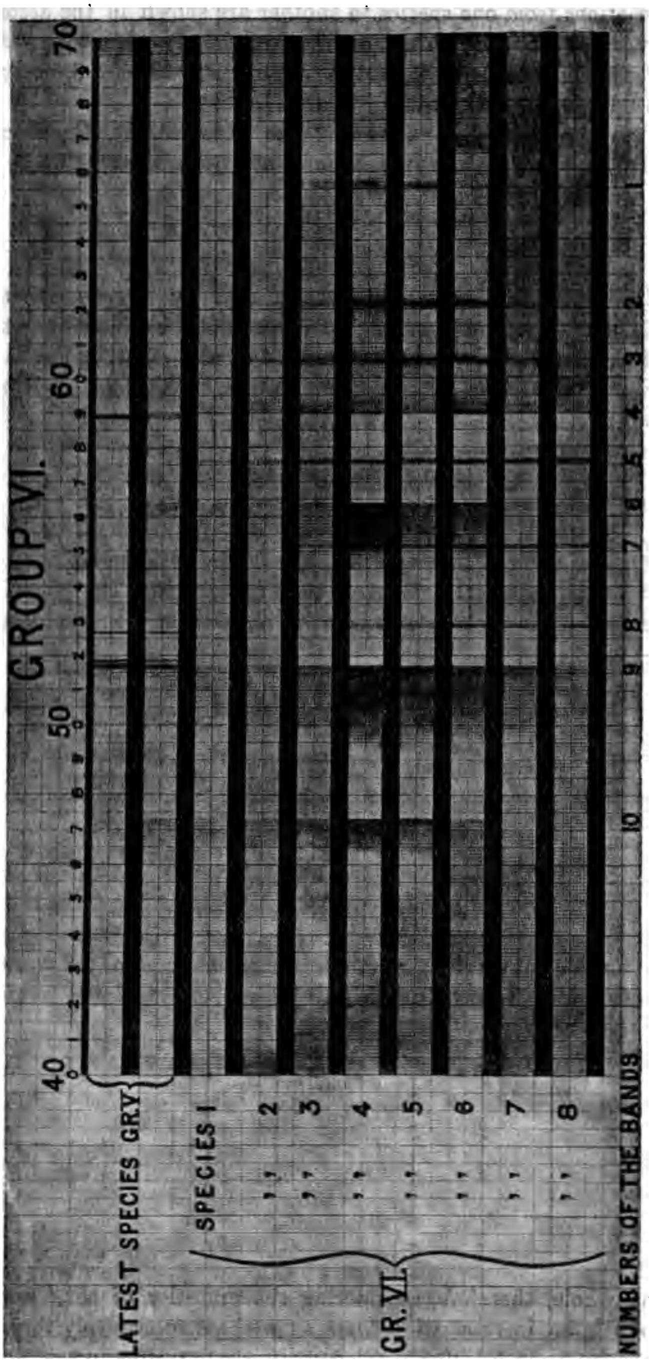
The numbers show a fair agreement with the letters employed, if we omit one Rrj star, which is given the number 9·3. The following table compares the colour numbers corresponding to the initial letters:—

Initial letters.	Range of numbers corresponding.	Mean No.	Remarks.
Rj	..	8·2	One member only.
Rrj	7·8—8·6	8·3	One star, 9·3, omitted.
Rrrj	8·8—9·5	9·04	

If we consider the colour numbers corresponding to the various species into which I have divided the group, we find the average numbers to be as follows:—

Species 1	....	8·1
2	....	8·6
3	....	8·2
4	....	8·1
5	....	8·2
6	....	8·8
7	....	8·6

On the whole, therefore, considering the difficulty of the observations, there is an increase of redness as we pass successively through



Map showing the Species of Piscian Stars.

successive species, which is exactly what we should expect if these species truly represent the effect of gradual cooling.

Variability of Piscian Stars.

Of the fifty-five stars in Dunér's list, ten exhibit fluctuations in brilliancy.

On the whole the light changes are not so great as in the stars of Group II, and the periods tend to greater length.

As to the cause of variability, the increase of light at maximum may be due, as I suggested in 1890,* to the light added by bodies of a cometary character when they reach periastron, the increase of luminosity being produced by tidal action, as in the case of comets in our own solar system. If there be any truth in this idea, it seems probable that the added light of the comet at maximum, which would give a spectrum consisting of bright carbon flutings, would produce a paling of the carbon absorption flutings.

As in the variables of the Antarian group, which are uncondensed swarms, and where, on the meteoritic hypothesis, the increased light at maximum is produced by the collision of a revolving swarm at periastron, irregularity is a natural consequence of the revolution of more than one secondary body.

ADDENDUM.

Recent Observations.

The Kensington observations were made chiefly during 1894 and 1895, with special reference to the lines involved. The stars selected for observation were 132 Schjellerup, 152 Schjellerup, 115 Schjellerup, and 19 Piscium. The 3-foot reflector was used. In addition to the carbon bands, numerous lines were seen without much difficulty, but only the more prominent ones could be satisfactorily measured. Among the lines recorded in 132 Schj. were $H\beta$, the E line of iron at 5269, and a group of lines near λ 5380. In 115 Schjellerup additional lines were measured near 5005, 5762, and 5429, and the presence of $H\beta$ was again determined by comparison with a hydrogen vacuum tube. In 19 Piscium numerous lines were observed, among them being D and F. No suspicion of bright lines was entertained during these observations. Attempts to photograph the spectra were not sufficiently successful to help matters.

In 1898, Dr. McClean published photographs of the spectra of 19 Piscium and 152 Schjellerup,† showing that these stars have a line spectrum similar to α Tauri, in addition to the well-marked bands

* 'Nature,' vol. 42, pp. 419, 548.

† 'Phil. Trans.,' vol. 191, A, p. 131, plate 14.

of carbon. This was the information wanted, but more recently Professor Hale has published photographs of the spectra of 280 Schj., 273 Schj. (19 Piscium), 132 Schj. (U Hydræ), and 152 Schj., taken with the aid of the Yerke's telescope at Chicago, and showing a wealth of fine detail.* The dark line spectrum is very marked, and the details of the carbon bands themselves are clearly revealed. Besides these, there are certain bright places in the spectrum which Professor Hale has been led to believe are true bright lines, and he mentions that Professor Keeler has arrived at the same conclusion as a result of his observations with the Lick refractor.

Dunér appears to have continued his observations of this group of stars after his removal from Lund to Upsala, and he states that with the Upsala refractor he was able to see more detail, and could detect without difficulty bright lines in the spectra of various stars of the group.†

The Question of Bright Lines.

As I have already pointed out, Professors Hale, Keeler, and Dunér consider that there are bright lines in some of these spectra, but I must confess that the published photographs do not convince me upon this point. In the plate which accompanies Professor Hale's paper of April, 1899,‡ the spectra of four stars are shown, namely, 280 Schj., 273 Schj., 132 Schj., and 152 Schj. A study of these photographs shows that the supposed bright lines are involved in the carbon absorption bands in the yellow green, and occur where there is reduced absorption, on the less refrangible sides of the dark flutings.

This at once led me to suppose that they could not be real bright lines, but simply places in the continuous spectrum where there is least absorption. These supposed bright lines are most marked in 152 Schj., and there is no suggestion of them in 280 Schj., while I think few would be disposed to suggest their presence in 273 Schj. and 132 Schj. without having 152 Schj. as a guide. Nevertheless, in these intermediate stars there are certainly bright *places* corresponding in position with the "bright lines" in 152 Schj., the principal one being at λ 5592. So far as appearances go, the greater apparent intensity of the bright line in 152 Schj. appears to be due to the introduction of a strong absorption line on the less refrangible side.

In another paper§ Professor Hale reproduces photographs of 152 Schj. in which the contrast has been increased by photographic means, so that the whole spectrum appears to consist of bright lines, rather than dark ones.

* 'Astrophys. Journ.,' vol. 8, pp. 238–9; vol. 9, p. 271; vol. 10, p. 110.

† 'Astrophys. Journ.,' vol. 9, p. 121.

‡ 'Astrophys. Journ.,' vol. 9, p. 271.

§ 'Astrophys. Journ.,' vol. 10, August, 1899, p. 108.

In favour of the real existence of bright lines, Professor Hale points out that the contrast between the line and the continuous spectrum increased rather than diminished when dispersion was increased, and that there was no decrease in contrast as the slit was widened. The question, however, is so complicated by the presence of the carbon fluting and other absorptions, that I shall not follow Professor Hale in his definite conclusions as to bright lines upon these grounds.

Before we can admit the certain presence of bright lines in 152 Schj., we must consider whether similar appearances occur in other stars where bright lines have not been previously suspected. As a matter of fact, in the photographic spectra of α Tauri, β Andromedæ, and α Orionis, which I published in 1893,* the spectra might, so far as mere appearance goes, be regarded as containing both bright and dark lines, some of the bright spaces between obvious dark lines being very conspicuous; the same remark applies in a less degree to the spectrum of Arcturus which I published at the same time. But we find a complete explanation of these spectra if we regard them as consisting of dark lines, whereas if we take the bright spaces we cannot match them at all. We do not hesitate in these cases to treat the spectra as consisting of dark lines only, the apparent bright lines being simply spaces between dark ones. I find that practically in all dark line spectra where the lines are from some cause or other thick, the intervals between them are apt to appear as bright lines, and this brightness can readily be intensified by purely photographic processes.

I have accordingly thought it unnecessary to modify the division into species on account of the supposed presence in some of them of bright lines. If the presence of bright lines be eventually established, may they not indicate that we are observing the effects of volcanic gases floating over a "photosphere" which has attained the consistency of lava?

Bearing on the Meteoritic Hypothesis.

The photographs taken by McClean and Hale have now sufficiently shown that there is much in common between the line spectra of the Antarian and Piscian stars. This indicates that there is a practical equality of mean temperature in the reversing layers of the two groups, but we find a very great difference in the conditions as to carbon; while carbon is undoubtedly absorbing in the Piscian stars, it is certainly not absorbing in the Antarian, and there is in fact strong evidence that it is radiating.†

We cannot imagine different kinds of stars of the same temperature as representing the same stage in any evolutionary scheme, so that the

* 'Phil. Trans.,' A, vol. 186 (1893), plate 23.

† 'Roy. Soc. Proc.,' vol. 44 (1888), p. 52; 'Phil. Trans.,' A, vol. 186 (1893), p. 704.

separation of the two groups which I suggested in 1887 is fully justified by the recent work to which I have referred. By putting the two groups on the same level of temperature, but on opposite sides of the temperature curve, as in the evolutionary order forming part of the meteoritic hypothesis, the differences are fully explained.

It will be seen that this work carries us a step beyond that with which I have recently been engaged in connection with the hotter stars.

General Conclusions.

(1) The undoubted presence of dark carbon flutings in the solar spectrum, including that near *b*, and of solar lines in the Piscian stars, indicates that the Piscian stars are next in order of development to the Arcturian stars.

(2) The stars observed by Dunér may be divided into seven species, beginning with the hottest and ending with the coolest stars.

(3) The reported presence of bright lines in the Piscian stars must be received with caution, as similar evidence of bright lines might be adduced in the case of other classes of stars in which the spectrum is fully explained by dark lines alone.

(4) The redness of the stars increases as we pass from the earlier to the later species of the group.

(5) The variability in this group is less marked than in the Antarian stars, and may perhaps be accounted for by the revolution of secondary bodies of the nature of comets round the stars themselves.

(6) The place on the temperature curve assigned to these stars on the meteoritic hypothesis is fully confirmed by the more detailed inquiry, and the hypothesis is thereby strengthened.

I am indebted to Mr. Fowler for assistance in the determination of the species and the construction of the map ten years ago, and for additional assistance in discussing the recent work. I have also to express my thanks to Mr. Shackleton for a detailed examination of the recent photographs.

"Mathematical Contributions to the Theory of Evolution.—On the Law of Reversion." By KARL PEARSON, F.R.S. (A New Year's Greeting to Francis Galton.—January 1, 1900.) Received December 28, 1899,—Read January 25, 1900.

(1) *Introductory.*—In a memoir recently presented to the Royal Society, I have endeavoured to emphasise the importance of distin-

guishing between three diverse types of heredity, namely (i), Blended Inheritance, (ii) Exclusive Inheritance, and (iii) Particulate Inheritance.

In a memoir printed in vol. 62, pp. 386—412 of the 'Proceedings,' I have dealt at length with the theory of blended inheritance, generalising for this purpose Mr. Galton's Law of Ancestral Heredity.

Allowing for a certain degree of variation in the constant γ, or "coefficient of heredity," there discussed, I consider that this theory gives a fairly good first approximation to the facts hitherto observed in this field. But blended inheritance certainly does not cover the whole field of heredity. When a character *blends*, then this law of ancestral heredity tells us the most probable blend for the offspring of given ancestry. It shows us the offspring of exceptional parents regressing towards mediocrity, owing to the fact that without stringent selection the great bulk of their ancestry must be mediocre and not exceptional.* Thus the main feature of the law of ancestral heredity is regression. Such regression is not what most biologists would understand by *reversion*. In fact, when the inheritance from a variety of ancestry is *blended*, the idea of reversion becomes very obscure; I venture to think meaningless.

Let us suppose stature a blended character, then the array of offspring of a definite *short* statured ancestry will have a mean regressing (here progressing) towards the population mean and a definite variability. Hence the theory of chance enables us at once to determine the frequency of a very tall man born of such short ancestry. The frequency may be small, but sooner or later the tall man will appear. Now let us suppose *one* distant ancestor in the otherwise short ancestry to have been tall. Clearly his existence will hardly affect at all the mean of the array of offspring.

He will not materially influence the chance of a very tall man appearing among the offspring; yet a superficial observer might easily describe the appearance of the very tall man as a case of reversion to the distant tall ancestor. The absurdity of this attribution is manifest when we remember that persons like him would have had sensibly equal frequency with or without the distant tall ancestor. In fact, it seems to me that in the case of characters which continuously vary, and which blend their inheritance, it is hopeless to look for any evidence whatever of reversion. The term is, then, meaningless.

To find reversion we must investigate cases in which characters do not blend, *i.e.*, the individual takes *exclusively* after some one member of the ancestry. In this case the appreciation of reversion becomes possible and its meaning intelligible. Cases of this kind are by no means un-

* An individual has 1024 10th great parents, and these can hardly be anything else but a fair sample of the population of their generation, if there has not been an excessive amount of in-and-in breeding or much selection.

common. Thus, Mr. Galton writes in his 'Natural Inheritance'(p. 139): "Parents of different statures usually transmit a blended heritage to their children, but parents of different eye-colours usually transmit an alternative heritage . . . . . . if one parent has a light eye-colour and the other a dark eye-colour, some of the children will, as a rule, be light and the rest dark; they will seldom be medium eye-coloured like the children of medium eye-coloured parents."

Again, in his paper on "Basset Hounds,"* Mr. Galton classifies these hounds as tricolour (T) and non-tricolour (N), remarking, "I am assured that transitional cases between T and N are very rare, and that experts would hardly ever disagree about the class to which any particular hound should be assigned." In other words, Mr. Galton appears to assume exclusive inheritance.† Roughly, in such exclusive inheritance, the offspring takes after one or other parent, or *reverts* to more distant ancestry. It becomes accordingly somewhat difficult to see how the law of ancestral heredity, which applies to blended inheritance, can be transferred to this different field. Yet Mr. Galton in his 'Natural Inheritance' (p. 153) writes: "The broad conclusion to which the present results irresistibly lead, is that the same peculiar hereditary relation that was shown to subsist between a man and each of his ancestors in respect to the quality of stature, also subsists in respect to that of eye-colour." Further, in the paper on Basset Hounds, he actually endeavours to demonstrate the truth of the law on the exclusive colour of these hounds. Now I think we must keep these two matters quite apart. The average stature of an individual is a blend of all his progenitors' characters; even in a single individual we find contributions from many ancestors; this is not the case with an exclusive inheritance, and it does not accordingly seem to me possible that "the same peculiar hereditary relation that was shown to subsist between a man and each of his ancestors" for a blended character can also hold for an exclusive character.

It is no longer of the proportions of a character in one individual that we speak, but of the frequency of various types of individuals among the total offspring of a given ancestry. The one statement is a law of blending characters, and the other is a law of distributing the exclusive characters among a group of individuals. In the first case we deal with regression, in the second with reversion. What Mr. Galton really asserts is, that the proportions of reversion in an array of offspring are identical with the proportions of blend in the average

* 'Roy. Soc. Proc.,' vol. 61, p. 403.

† A remark in the 'Natural Inheritance' (p. 139) that "Stature is due to its being the aggregate of the quasi-independent inheritances of many separate parts, while eye-colour appears to be much less various in its origin," would seem to indicate that Mr. Galton considers that blended inheritance is ultimately based upon exclusive inheritance of parts—a suggestion well worth investigation.

individual. If this be true, then his law, or possibly some generalisation of it, is very comprehensive; it embraces the two distinct types of heritage, blended and exclusive. But I think it most desirable to keep the two ideas quite separate, and speak of the one dealing with blended inheritance as the *Law of Ancestral Heredity;* the second, dealing with exclusive inheritance, as the *Law of Reversion.* If this be done, we shall, I venture to think, keep not only our minds, but our points for observation, clearer; and further, the failure of Mr. Galton's statement in the one case will not in the least affect its validity in the other.

(2) *The Law of Reversion.*—Let us examine first what I take to be Mr. Galton's view of this law. Out of an array of N offspring, 1/4 N will follow each parent, 1/16 N follow each of the four grandparents, and $\frac{1}{2^{2n}}$N follow each of the 2^n nth great parents. In this manner the total offspring N is distributed by reversion among the ancestry.

Now I want to draw attention to one or two points here. 1/4 N will not be all the children like, say, their father; for out of the 1/4 N who are like members of his ancestry, those who are like ancestors like him—and these ancestors will occur in certain proportions—will thus also be like him. This holds for each individual ancestor; the number *like* any ancestor will be considerably greater than the number who "follow" that special ancestor. Now let ρ_1N, ρ_2N, ρ_3N, ρ_4N, and ...... ρ_nN... be the number of the offspring like a parent, a grandparent, a great grandparent, and nth parent, &c.

This brings me to my second point. A special meaning is here given to the word *like.* ρ_1N is not in the usual sense of the word *all* the number like the father. If the offspring had the same distribution of character as we find in the general population, then undoubtedly some would have the same quantity or quality of the character as he has—some, for instance, would be blue-eyed if he were blue-eyed—but this is a random likeness and not like in the special sense in which we are using the word. ρ_1N are like the father owing to the laws of heredity, the remainder have a random distribution so far as he is concerned, and we exclude any random likeness from our consideration.

How then are we in actual observation to distinguish hereditary from random likeness?* The answer is simple; ρ_1N out of N pairs of parent and offspring will be absolutely correlated, *i.e.*, have a correlation equal to unity, but the remaining $(1-\rho_1)\times$N pairs will have zero correlation, although there may be random likenesses. Hence, by the theorem given by me in the 'Phil. Trans.,' vol. 192, p. 276, the actual correlation will be perfect correlation reduced in the ratio of the

* I exclude for the present the influence of assortative mating. A likeness to the mother, otherwise random so far as the father is concerned, may thus become a real likeness to the father.

number of correlated pairs to the total number of pairs.* Thus the correlation of parent and offspring $= 1 \times \rho_1 N/N = \rho_1$.

It thus follows that $\rho_1, \rho_2, \rho_3 \ldots\ldots, \rho_n \ldots\ldots$ are the correlation coefficients to be expected between offspring and parent, grandparent, ...nth great parent, &c. Here we have assumed equal potency for both sexes and all lines of descent, otherwise these coefficients must be looked upon as mean values of the correlations for different generations of ancestry.

Lastly, it seems to me that reversion may not be the proper word to apply to those who directly follow their parents, and that these may be fairly considered direct inheritors and distinguished from reverters. I shall accordingly assume no *à priori* relation between these two classes, certainly not that direct inheritors and reverters are equally numerous, *i.e.*, $\frac{1}{2}$ and $\frac{1}{2}$, as in Mr. Galton's Law. As for reversion itself I will only suppose it to diminish in geometrical progression as we step backward to more and more distant ancestry. I shall accordingly take βN offspring to follow either parent and $\gamma\alpha N$, $\gamma\alpha^2 N$, $\gamma\alpha^3 N$, &c., to follow grandparent, great grandparent, great great grandparent, &c. With these preliminaries arranged we can now proceed with the analysis.

(3) *The Generalised Law of Reversion.*—The total number of offspring N is clearly the sum of all those that follow all the successive ancestors, *i.e.*,

$$N = 2\beta N + 4\gamma\alpha N + 8\gamma\alpha^2 N + 16\gamma\alpha^3 N + \ldots\ldots$$

or

$$1 = 2\beta + 4\gamma\alpha/(1 - 2\alpha) \ldots\ldots \quad \text{(i)}$$

Now consider how the number of offspring "like" or absolutely correlated with one parent are made up: they are $\rho_1 N$ in number; they consist first of βN, the number directly inheriting from this parent; also there will be $\gamma\alpha N$ like each of the parent's parents, and the parent will be like one or other of the parent's parents in ρ_1 proportion of cases; similarly there will be $\gamma\alpha^2 N$ like each of the parents' grandparents, and the parent is like each of the parents' grandparents in ρ_2 cases; and so on. Thus we have

$$\rho_1 N = \beta N + 2\gamma\alpha\rho_1 N + 4\gamma\alpha^2\rho_2 N + 8\gamma\alpha^3\rho_3 N + \ldots\ldots$$

or

$$\rho_1 = \beta + 2\gamma\alpha(\rho_1 + 2\alpha\rho_2 + 4\alpha^2\rho_3 + \ldots\ldots) \ldots\ldots \quad \text{(ii)}$$

Now note how the $\rho_2 N$ like any one grandparent is made up. We have directly $\gamma\alpha N$ reverting to this grandparent, $\gamma\alpha^2 N$ to each of the grandparents' parents, and in each case $\rho_1\gamma\alpha^2 N$ like the grandparent; similarly out of those $\gamma\alpha^3 N$ reverting to any grandparents' grand-

* In this case there is every reason for supposing $\sigma_1 = \sigma_2 = \sigma_1' = \sigma_2'$, and $m_1 = m_2$, $m_1' = m_2'$. Thus $\Sigma = \sigma_1' \Sigma' = \sigma_1'$, and since $r = 1$, $R = n_1/N$.

parent, there will be $\rho_2\gamma\alpha^3N$ like the grandparent, and so on. But beyond these contributions, certain of the βN who follow the parent will be like the grandparent, for the parent is like the grandparent in ρ_1 fraction of cases.

Hence we have finally:

$$\rho_2 N = \rho_1\beta N + \gamma\alpha N + 2\rho_1\gamma\alpha^2 N + 4\rho_2\gamma\alpha^3 N + \ldots\ldots$$

or

$$\rho_2 = \rho_1\beta + \gamma\alpha + 2\gamma\alpha^2(\rho_1 + 2\alpha\rho_2 + 4\alpha^2\rho_3 + \ldots\ldots)\ldots\ldots\ldots\ldots\text{(iii)}$$

Proceeding in the same manner we find

$$\rho_3 = \rho_2\beta + \gamma\alpha\rho_1 + \gamma\alpha^2 + 2\gamma\alpha^3(\rho_1 + 2\alpha\rho_2 + 4\alpha^3\rho_3 + \ldots\ldots)\ldots\ldots\ldots\text{(iv)}$$

$$\rho_4 = \rho_3\beta + \gamma\alpha\rho_2 + \gamma\alpha^2\rho_1 + \gamma\alpha^3 + 2\gamma\alpha^4(\rho_1 + 2\alpha\rho_2 + 4\alpha^3\rho_3 + \ldots)\ldots\text{(v)}$$

and so on.

Hence we deduce from (iii) and (ii)

$$\rho_2 = \rho_1\beta + \gamma\alpha + \alpha(\rho_1 - \beta)$$

or

$$\rho_2 = (\alpha+\beta)\rho_1 + \alpha(\gamma - \beta)\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\text{(vi)}$$

Similarly from (iv) and (iii)

$$\rho_3 = \rho_2\beta + \gamma\alpha\rho_1 + \alpha(\rho_2 - \rho_1\beta)$$

or

$$\rho_3 = (\alpha+\beta)\rho_2 + \alpha(\gamma-\beta)\rho_1\ \ldots\ldots\ldots\ldots\ldots\text{(vii)}$$

Again from (v) and (iv)

$$\rho_4 = \rho_3\beta + \gamma\alpha\rho_2 + \alpha(\rho_3 - \rho_2\beta)$$

or

$$\rho_4 = (\alpha+\beta)\rho_3 + \alpha(\gamma-\beta)\rho_2\ \ldots\ldots\ldots\ldots\ldots\ldots\text{(viii)}$$

Generally

$$\rho_n = (\alpha+\beta)\rho_{n-1} + \alpha(\gamma-\beta)\rho_{n-2}\ \ldots\ldots\ldots\ldots\ldots\ldots\text{(ix)}$$

with $\rho_0 = 1$, by (vi).

To solve equation (ix) assume as usual $\rho_n = Am^n$, and we find

$$m^2 - (\alpha+\beta)m + \alpha(\gamma-\beta) = 0.$$

Thus

$$m = \frac{\alpha+\beta \pm \sqrt{(\alpha+\beta)^2 + 4\alpha(\gamma-\beta)}}{2}.$$

But by (i)

$$1 - 2(\alpha+\beta) = 4\alpha(\gamma-\beta).$$

Hence

$$m = \frac{1}{2} \quad\text{or}\quad m = \alpha+\beta-\frac{1}{2}\ \ldots\ldots\ldots\ldots\ldots\ldots\text{(x)}$$

We have then

$$\rho_n = A_1\left(\frac{1}{2}\right)^n + A_2\left(\alpha+\beta-\frac{1}{2}\right)^n\ \ldots\ldots\ldots\ldots\ldots\ldots\text{(xi)}$$

where A_1 and A_2 are constants.

But $\rho_0 = 1$, $\therefore A_1 + A_2 = 1$. Thus we may put

$$\rho_n = (1-c)(\tfrac{1}{2})^n + c(\alpha+\beta-\tfrac{1}{2})^n \quad \ldots\ldots\ldots\ldots \quad \ldots. \text{(xii)}$$

where c is a constant.

Let us substitute this in equation (ii); we find

$$(1-c)\frac{1}{2} + c\left(\alpha+\beta-\frac{1}{2}\right)$$

$$= \beta + 2\gamma\alpha\left\{(1-c)\frac{1}{2} + 2\alpha(1-c)\frac{1}{2^2} + 4\alpha^2(1-c)\frac{1}{2^3} + \ldots\ldots\right.$$

$$\left. + c\left(\alpha+\beta-\frac{1}{2}\right) + c\left(\alpha+\beta-\frac{1}{2}\right)^2 2\alpha + c\left(\alpha+\beta-\frac{1}{2}\right)^3 4\alpha^2 + \ldots\ldots\right\}$$

or
$$= \beta + 2\gamma\alpha\left\{\frac{\frac{1}{2}(1-c)}{1-\alpha} + \frac{c(\alpha+\beta-\frac{1}{2})}{1-2\alpha(\alpha+\beta-\frac{1}{2})}\right\} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\text{(xiii)}$$

Write $\delta = \alpha+\beta-\frac{1}{2}$, then by (i) we have

$$\alpha\gamma = \tfrac{1}{2}(\alpha-\delta)(1-2\alpha) \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(xiv)}$$

Hence from (xiii)

$$c\left(\delta - \frac{2\gamma\alpha\delta}{1-2\alpha\delta} + \frac{\gamma\alpha}{1-\alpha} - \frac{1}{2}\right) = \delta - \alpha + \frac{\gamma\alpha}{1-\alpha}$$

and by (xiv)

$$c = \frac{(1-2\alpha\delta)(\delta-\alpha)}{(1-2\delta)\{(\alpha-\delta)(1-2\alpha)-(1-\alpha)(1-2\alpha\delta)\}} \ldots\ldots\ldots\ldots \text{(xv)}$$

Suppose the parental and grandparental correlations observed, then

$$\left.\begin{aligned}\rho_1 &= \tfrac{1}{2}(1-c)+c\delta \\ \rho_2 &= \tfrac{1}{4}(1-c)+c\delta^2\end{aligned}\right\}\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(xvi)}$$

will both be known.

These will give c and δ; then (xv) will give α and (xiv) γ, while

$$\beta = \delta - \alpha + \tfrac{1}{2} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(xvii)}$$

will determine β, and the whole law of inheritance and reversion will have its constants fully determined.

We have, indeed, from (xvi)

$$\delta = \frac{\rho_1 - 2\rho_2}{1-2\rho_1} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(xviii)}$$

$$c = \frac{(1-2\rho_1)^2}{1-4\rho_1+4\rho_2} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(xix)}$$

From (xv)

$$\alpha^2 - \frac{2+e+4\delta+2e\delta^2}{2(1+\delta+e\delta)}\alpha + \tfrac{1}{2} = 0 \quad \text{.................. (xx)}$$

where
$$e = \frac{1}{1-2\rho_1}.$$

Lastly from (xiv)
$$\gamma = \frac{(\alpha-\delta)(1-2\alpha)}{2\alpha} \quad \text{.......................... (xxi)}$$

Thus (xviii) and (xix) give δ and c. (xx) then gives α, taking the root *less than unity*. Finally (xvii) and (xxi) give β and γ, completing the solution.

(4) *Comparison with Law of Ancestral Heredity.*—Now let us compare these results with those I have obtained from the law of ancestral heredity.* On p. 390 of the memoir on that subject we have for the nth midparental correlation with the offspring $\rho_n = 2^{\frac{1}{2}n} r_n$, where r_n is the correlation of the offspring with the individual nth great grandparent. By p. 394 $\rho_n = c\alpha^n$. Hence

$$r_n = c(\alpha/\sqrt{2})^n \quad \text{.......................... (xxii)}$$

or the correlations of the offspring with the ancestry follow a simple geometrical progression.

Comparing this with the result (xii) of this paper, or

$$r_n = (1-c)(\tfrac{1}{2})^n + c(\alpha+\beta-\tfrac{1}{2})^n \quad \text{........... (xii)}$$

where c is now a different constant, we see that the two cannot possibly be in agreement, unless one of the terms of the latter result vanishes. Thus there is in general a fundamental difference between the law of ancestral heredity and the law of reversion; they give expressions differing in character for the correlations between the offspring and individual ancestors. Let us see when the two laws will agree. There is unfortunately a bad slip in my memoir of 1898. The series at the top of p. 403 leads to

$$\gamma\beta'/(1-\beta') = 1 \quad \text{and not as there given} \quad \gamma\beta'(1-\gamma\beta') = 1.$$

Thus we have $\quad \beta'(1+\gamma) = 1 \quad$ or $\quad \beta' = 1/(1+\gamma)$.

Hence
$$\beta = \frac{1}{\sqrt{2}}\beta' = 1/\{\sqrt{2}(1+\gamma)\} \quad \text{........... (xxiii)}$$

and therefore by (xii) $\qquad \alpha = 1/\sqrt{2}$.

Thus by (xxii) of this paper

$$r_n = c(\tfrac{1}{2})^n \quad \text{......... .... (xxiv)}$$

* 'Roy. Soc. Proc.,' vol. 62, pp. 386—412.

This shows us that the correlation with the individual ancestor is halved at each backward step in the pedigree. We see at once that (xii) can only be in agreement with (xxiv)—the letter c being different in the two, and merely standing for a constant—provided $\alpha+\beta = \frac{1}{2}$, or by (i), provided $\gamma = \beta$. Thus the condition that blended and exclusive inheritance should lead to the same values for the correlations with the ancestry is: that reversion should form a series starting with the actual parent. If this condition should hold, then, for example, the grandparental correlation must always be one-half the parental either for blended or for exclusive inheritance.

(5) *Correction of an Error in Memoir on Ancestral Heredity.*—It may be of value to insert here the modifications required in my memoir on blended inheritance, owing to the slip just referred to: they apply to the results deduced from (xvii) on p. 403; these are the table on p. 403, and the result immediately under (xviii) on p. 406.

In the first place the law of ancestral heredity may now be written

$$k_0 = \frac{\gamma}{1+\gamma}\frac{\sigma_0}{\sigma_1}k_1 + \frac{\gamma}{(1+\gamma)^2}\frac{\sigma_0}{\sigma_2}k_2 + \frac{\gamma}{(1+\gamma)^3}\frac{\sigma_0}{\sigma_3}k_3 + \ldots\ldots\ldots\ldots \quad \text{(xxv)}$$

a very simple form.

In the second place we may replace the equation for c on p. 394 by

$$c = \gamma\frac{2\gamma+1}{2\gamma^2+2\gamma+1} \ldots\ldots\ldots\ldots \quad \text{(xxvi)}$$

whence we find for the parental correlation $c\alpha/\sqrt{2}$ or $\frac{1}{2}c$

$$r_1 = \tfrac{1}{2}\gamma\frac{2\gamma+1}{2\gamma^2+2\gamma+1} \ldots\ldots\ldots\ldots \quad \text{(xxvii)}.$$

Each succeeding ancestral correlation will be obtained by repeated halving of this value.

Lastly, the result on p. 406 for the fraternal correlation becomes*—

$$r = \frac{2\gamma^2}{2\gamma^2+2\gamma+1} \ldots\ldots\ldots\ldots \quad \text{(xxviii)}.$$

The following table indicates the effect of varying γ on the intensity of heredity, and should replace that on p. 403 of the memoir on the ancestral law:—

* Equation (xviii) on p. 406 of the memoir is correct, but the value of r in terms of γ below it, since it depends on the erroneous Equation (xvii) of p. 403, as well as the limit given for γ in the foot-note, must be cancelled.

Table of Heredity for divers Values of γ.

Value of γ.	0·7.	0·9.	1.	1·2.	2·35.	∞.
Parental correlation	0·2485	0·2851	0·3000	0·3248	0·4000	0·5000
Grandparental correlation ...	0·1243	0·1425	0·1500	0·1624	0·2000	0·2500
Great grandparental correlation	0·0621	0·0713	0·0750	0·0812	0·1000	0·1750
Fraternal correlation	0·2899	0·3665	0·4000	0·4586	0·6596	1·0000
Regression on nth midparent	0·4970	0·5701	0·6000	0·6497	0·7999	1·0000
Correlation with nth midparent	$0{\cdot}4970(0{\cdot}7071)^n$	$0{\cdot}5701(0{\cdot}7071)^n$	$0{\cdot}6000(0{\cdot}7071)^n$	$0{\cdot}6497(0{\cdot}7071)^n$	$0{\cdot}7999(0{\cdot}7071)^n$	$(0{\cdot}7071)^n$

(6) *Difficulties arising when we apply these Results for Blended Inheritance.*—Now the above table shows us that by varying γ sufficiently we can obtain a considerable range of values for the correlation of characters in kindred. But these values are limited by two serious considerations, namely :—

(i) The ancestral correlation is halved at each stage.

(ii) The fraternal correlation appears to become perfect as we approach the upper limit of parental correlation, *i.e.*, 0·5.

Now actual determinations of grandparental correlation in the cases of eye-colour in man, of coat-colour in horses, and of coat-colour in hounds, which I have recently made, do not as a rule seem to justify the statement that the grandparental is half the parental correlation. Further, in two of these cases, the average parental correlation is quite 0·5, but the fraternal correlation is, while larger than 0·4, still a good deal short of perfect. Hence I am bound to conclude that :—

(i) These characters do not obey the laws of blended inheritance as deduced from the law of ancestral heredity ; or,

(ii) The laws of blended inheritance, as deduced from the law of ancestral heredity, would be largely modified if we considered the influence of assortative mating, or

(iii) The fundamental assumption that if all the midparents right away back had the same amount of the character, the average offspring would have also the same amount, is not justified. Thus the result $\beta = 1/(\sqrt{2}(1+\gamma))$ in Equation (xxiii), perhaps, is unnecessary, or there may be *two* independent constants of inheritance.

It is quite possible that eye-colour in man and coat-colour in hounds are exclusive and not blended inheritances, so that (i) would cover these cases. On the other hand, I have found parental correlations as high as 0·5 for a new and large series of stature data in man, without fraternal correlation approaching unity. Here (i) can hardly apply, although (ii) may, for the coefficient of assortative mating in this case is remarkably high, nearly 0·3. But I think that, even if (i) or (ii) might help us over our difficulties in certain cases, we ought to carefully reconsider the assumption referred to in (iii). It would surely only be justifiable in the case of an absolutely stable population, each generation of which has existed under an identical environment. In itself it seems to exclude any secular change due to natural selection, or to improved physical or organic environment. In fact, we must proceed with caution when applying the statement that the average of all the offspring of an absolutely same system of midparents would be like those midparents; for a portion of such offspring have very probably been removed by selection, and our average is not really that of *all* the offspring, but of the fitter. In the like manner, we must treat with some caution the principle on which Equation (i) of the present paper is based. It assumes that all the ancestral contributions are to be found in the present progeny; but what if the contributions of certain ancestors by selection, artificial or natural, have been eliminated before reaching the existing generation? What if the coat-colours of certain ancestors were unfashionable, and only their unlike descendants have been put to the stud? Our theory may be quite correct, but it may appear erroneous when tested by facts observed in the case of horse or dog breeding.

Let us investigate whether independent γ and β in our expressions for parental and fraternal correlations would enable us in the case of blended inheritance to reach a value of the former as high as 0·5 without the latter becoming perfect. I find if r_1 be the parental correlation, $= c\alpha/\sqrt{2}$, from Equation (xiii) of my memoir on the ancestral law (p. 394), and if r be the fraternal correlation obtained from Equation (xviii) of the same memoir:

$$r_1 = \frac{\gamma\beta}{\sqrt{2}} \frac{1-\beta^2(1+\gamma)}{1-\beta^2(1+2\gamma)} \quad \text{(xxix)}.$$

$$r = \frac{2\gamma^2\beta^2}{1-\beta^2(1+2\gamma)} \quad \text{(xxx)}.$$

Whence, eliminating $\gamma\beta$, we have

and

$$\left.\begin{aligned} \beta^2 &= \frac{2(r-4r_1^2)}{r(2-r)} \\ \gamma &= \frac{\sqrt{2}\,r_1}{\beta} - \tfrac{1}{2}r \end{aligned}\right\} \quad \text{(xxxi)}.$$

These give γ and β when the parental and fraternal correlations are known.

Now, since r is <1, β^2 will be imaginary, if r be not $>4r_1^2$. Hence we should again need perfect fraternal correlation for r_1 to be as large as 0·5.

Thus with blended inheritance and little or no assortative mating we cannot get a parental correlation as high as the value 0·5, which actually does occur in my data for both men and horses.

We must now consider how the problem will be affected, if we suppose exclusive and not blended inheritance.

(7) *Illustrations of the Law of Reversion in Exclusive Inheritance.*—(i) Let us first consider what happens if we take the chief feature of Mr. Galton's view, *i.e.*, that the likeness to the parent is the beginning, so to speak, of the reversion series. Then $\gamma = \beta$ (in the notation of the present memoir). It follows from Equation (i) that:

$1 - 2(\alpha + \beta) = 0$, or $\delta = 0$. Thus by (xviii)

$$\rho_1 = 2\rho_2,$$

and generally

$$\rho_n = 2\rho_{n-1}.$$

Equation (xx) to find α now becomes

$$\alpha^2 - (1 + \tfrac{1}{2}e)\alpha + \tfrac{1}{2} = 0,$$

while

$$\gamma = \tfrac{1}{2} - \alpha.$$

Thus as soon as we know ρ_1, we can find all the ancestral correlations and the whole series of reversions. For example: if $\rho_1 = 0{\cdot}4$ we should have $\rho_2 = 0{\cdot}2$, $e = 5$, and $\alpha^2 - 3{\cdot}5\,\alpha + 0{\cdot}5 = 0$. $\therefore \alpha = 0{\cdot}149$ and $\gamma = 0{\cdot}351 = \beta$. Thus in this case 35 per cent. of the offspring take after each parent, and 30 per cent. revert to higher ancestry. Of this 30 per cent. $100 \times 0{\cdot}35 \times 0{\cdot}149$, or 5·23 per cent., revert to each of the four grandparents, leaving 9 per cent., about, to revert to great grandparents and higher ancestry still.

(ii) Next suppose Mr. Galton's full view to be correct, and that 1/4 of the offspring follow each parent, 1/16 each grandparent, 1/64 each great grandparent, and so on. Then we have—

$$\alpha = \gamma = \beta = \tfrac{1}{4}.$$

Hence from $\alpha^2 - (1 + \frac{1}{2}e)\alpha + \frac{1}{2} = 0$, we find

$$e = 2{\cdot}5 \quad \text{and} \quad \rho_1 = 0{\cdot}3.$$

Thus we should have: $\rho_1 = 0{\cdot}3$, $\rho_2 = 0{\cdot}15$, $\rho_3 = 0{\cdot}075$, &c., *or, precisely the same ancestral correlations in the case of exclusive that we have in the case of blended inheritance by the law of ancestral heredity for the special case of* $\gamma = 1$ (see table, p. 149).

Thus the law of reversion fits no better than the law of blended inheritance the data to which I have referred (in § 6) when we adopt the 1/4, 1/16, 1/64 hypothesis, *i.e.*, the original form of Mr. Galton's statement.*

(iii) Let us suppose the parental correlation to be 0·5, a value not very far from what I have found for eye-colour in man and coat-colour in horses. Then by (xvi)

$$\delta = \tfrac{1}{2}, \quad \rho_2 = \tfrac{1}{4}.$$

Putting $e = \infty$ in (xx) we deduce:

$$\alpha^2 - \frac{1+2\delta^2}{2\delta}\alpha + \tfrac{1}{2} = 0, \quad \text{or} \quad \alpha^2 - \tfrac{3}{2}\alpha + \tfrac{1}{2} = 0,$$

which gives us $\alpha = 1$ or $\frac{1}{2}$.

But remembering the value of δ we have, using (xxi),

$$\alpha\gamma = -(\alpha - \tfrac{1}{2})^2 \quad \text{and} \quad \alpha + \beta = 1.$$

The first equation shows us that $\alpha = 1$ is impossible, for it gives γ negative. Accordingly we conclude that $\alpha = \frac{1}{2}$ and $\beta = \frac{1}{2}$, while $\gamma = 0$. *Thus reversion is totally excluded and one-half the offspring take after each parent.* In this case the grandparental correlation, ρ_2, is 0·25, the great grandparental 0·125, and so on. The ancestry beyond the parents have no direct influence on the offspring, beyond the fact that they have determined the parents. We are dealing indeed with a case like that investigated in my memoir on "Regression, Heredity, and Panmixia."† So far our theory of exclusive inheritance with parental correlation = 0·5 agrees with that of blended inheritance with the same value of the parental correlation. But we have seen that the latter leads to an impossible value for fraternal correlation, *i.e.*, one which does not fit the facts. Does perfect fraternal correlation necessarily flow from exclusive inheritance without reversion? Certainly not, for this would connote that all the offspring of a given set of parents would be alike, or one parent in each family be absolutely prepotent. This is of course not the fact.

Supposing all families to consist of n members, and that both parents were equipotent in the family, there would be $2\frac{n}{2}\left(\frac{n}{2} - 1\right)$ pairs of brethren alike, out of a total of $\frac{n(n-1)}{2}$ pairs, or the fraternal correlation would be $(\frac{1}{2}n - 1)/(n - 1)$. The average size of a human

* See 'Natural Inheritance,' chapter viii, p 149, &c. Mr. Galton there uses the correlation coefficients corresponding to blended inheritance for eye-colour, an exclusive inheritance. But, directly investigated, such values are far from holding for eye-colour.

† 'Phil. Trans.,' vol. 187, p. 303.

family is about 4·5; but if we confine ourselves to one sex, we must exclude all sterile marriages and all not leading to two brothers, or two sisters. We might then very well take $n = 6$, which gives 0·4 for the fraternal correlation. Thus we might expect in the case of exclusive inheritance that the fraternal correlation would lie between 0·4 and 1, according as to the degree of prepotency of one or other parent in the individual marriage.*

Thus our theory of exclusive inheritance is not, like that of blended inheritance, incompatible with observed facts, *i.e.*, high values of parental correlation and values substantially less than unity of the fraternal correlation. But for such cases we must deny the existence of any regular and continuous law of reversion. We should have to look upon reversion, if it occurred at all, as merely an irregular and infrequent phenomenon.

On the other hand, if we differentiate the taking after parents from the reversion to ancestry as phenomena of a quite distinct nature, our theory will enable us to surmount, for some cases at least, those difficulties in ancestral correlation, which arise when we take Mr. Galton's Law in its original form to cover both blended and exclusive inheritance. I illustrate this from data for the coat-colour of Basset Hounds in the following section.

(8) *Application to Basset Hounds.*—Understanding that I was desirous of testing my theory on a character which was definitely exclusive, Mr. Galton, with his invariable kindness, at once placed at my disposal his material on Basset Hounds. The reader will remember from the statements in Mr. Galton's own memoir ('Roy. Soc. Proc.,' vol. 61, p. 403) that these dwarf bloodhounds are either lemon and white, or black, lemon, and white; and here, as in Mr. Galton's work, they will be classified as non-tricolour and tricolour, or by the symbols N and T. In dealing with the offspring I was in many cases unable to determine the sex of the dog, as that information is not in the stud book,† and all individuals are not again recorded as sires or dams, nor do they possess obviously male or female names. Thus in my principal tables all the offspring of both sexes are clubbed together. To measure the legitimacy of this, I have formed separate tables of the two sexes in the case of sires and dams. Further, in dealing with great grandparents there were so few of each of the eight individual types alone, that I have formed merely one table, that of great grandparent and offspring, disregarding the line of descent.

* The mother and father may be equipotent on the average, but in the individual family one or other be markedly prepotent. It is to this prepotency of the individual, regardless of sex, that the increase of fraternal correlation beyond 0·4 is very probably due.

† Sir Everett Millais, 'The Basset Hounds Club Rules and Stud Book,' 1874—1896.

Tables of Inheritance of Coat-colour in Basset Hounds.

Sire and Offspring.

(1) All Offspring.

		T.	N.T.	Totals.
Sires.	T.	477	236	713
	N.T.	63	53	116
	Totals	540	289	829

(2) ♂ Offspring.

		T.	N.T.	Totals.
Sires.	T.	248	113	361
	N.T.	31	28	59
	Totals	279	141	420

(3) ♀ Offspring.

		T.	N.T.	Totals.
Sires.	T.	192	103	295
	N.T.	24	18	42
	Totals	216	121	337

Dam and Offspring.

(4) All Offspring.

		T.	N.T.	Totals.
Dams.	T.	369	94	463
	N.T.	169	191	360
	Totals	538	285	823

(5) ♂ Offspring.

		T.	N.T.	Totals.
Dams.	T.	195	46	241
	N.T.	93	92	185
	Totals	288	138	426

(6) ♀ Offspring.

		T.	N.T.	Totals.
Dams.	T.	146	41	187
	N.T.	64	85	149
	Totals	210	126	336

Sire's Sire and Offspring.

(7) All Offspring.

Sire's Sires.	T.	N.T.	Totals.
T.	365	157	522
N.T.	28	24	52
Totals	393	181	574

Sire's Dam and Offspring.

(8) All Offspring.

Sire's Dams.	T.	N.T.	Totals.
T.	266	112	378
N.T.	128	70	198
Totals	394	182	576

Dam's Sire and Offspring.

(9) All Offspring.

Dam's Sires.	T.	N.T.	Totals.
T.	344	156	500
N.T.	49	22	71
Totals	393	178	571

Dam's Dam and Offspring.

(10) All Offspring.

Dam's Dams.	T.	N.T.	Totals.
T.	233	81	314
N.T.	161	101	262
Totals	394	182	576

Great grandparent and Offspring.

(11) All Offspring.

Great grandparent.	T.	N.T.	Totals.
T.	1220	404	1624
N.T.	461	171	632
Totals	1681	575	2256

Whole Siblings (same litter).

(12) First Sibling.

Second Sibling.	T.	N.T.	Totals.
T.	942	317	1259
N.T.	317	448	765
Totals	1259	765	2024

Whole Siblings (different litters).

(13) First Sibling.

Second Sibling.	T.	N.T.	Totals.
T.	324	108	432
N.T.	108	160	268
Totals	432	268	700

Half Siblings (Dam's side).

(14) First Sibling.

Second Sibling.	T.	N.T.	Totals.
T.	1766	842	2608
N.T.	842	722	1564
Totals	2608	1564	4172

Half Siblings (Sire's side).

(15) First Sibling.

Second Sibling.	T.	N.T.	Totals.
T.	8898	4828	13726
N.T.	4828	3466	8294
Totals	13726	8294	22020

The extraction of eleven tables from Mr. Galton's data papers I owe to Miss Alice Lee, D.Sc. A twelfth is due to Mr. K. Tressler. For the other three I am responsible. Of the determinations of the correlation coefficients, I owe five to Miss Alice Lee, no less than six to Mr. L. N. G. Filon, M.A., and the remaining four only are my own work. The method by which the correlation coefficients have been calculated will be explained and justified in another memoir; it is a novel process, which we believe to be of considerable importance, and which we have already applied to a variety of attributes not capable of exact quantitative measurement. The probable error of this method of determining the correlation has also been ascertained, and may be taken in the present cases to range from 0·01 to 0·04, so that differences which are significant can be appreciated.

Coefficients of Correlation.

Sire and offspring :—		
Sire and all offspring		0·1775
Sire and ♂ offspring	0·2280	
Sire and ♀ offspring	0·1104	
Dam and offspring :—		
Dam and all offspring		0·5239
Dam and ♂ offspring	0·5077	
Dam and ♀ offspring	0·5445	
Mean parental correlation		ρ_1 = 0·3507
Grandparent and offspring :—		
Sire's sire and all offspring	0·2144	
Sire's dam and all offspring	0·0976	
Dam's sire and all offspring	−0·0032	
Dam's dam and all offspring	0·2215	
Mean grandparental correlation		ρ_2 = 0·1326
Great grandparent and offspring :—		
All great grandparents and all offspring	0·0404	ρ_3 = 0·0404
Siblings* :—		
Whole siblings from same litter	0·5084	
Whole siblings from different litters	0·5257	
Half siblings, dam's side	0·2222	
Half siblings, sire's side	0·1070	

* Every writer on heredity must have felt the inconvenience of our language having preserved no word for either member of a pair of offspring of either or both sexes from the same parent. After some hesitation, I have ventured to reintroduce a good Anglo-Saxon word with this sense.

Now several noteworthy conclusions follow at once from these numbers :—

(1) *Direct Inheritance.*

(*a*) The dam has a great prepotency in the matter of coat-colour. Mr. Galton has already remarked on this.* We here see that, quantitatively, the dam is, on the average of both sexes, thrice as highly correlated with the offspring as the sire. While she has reached the high value 0·5, he has fallen below 0·2, and the theoretical value 0·3 of the unmodified law of ancestral heredity is neither satisfactory for the individual cases nor for their average.

(*b*) Offspring take more after the dam than the sire, but ♂ offspring more than ♀ after the sire, and ♀ offspring more than ♂ after the dam. In other words, the parent hands down its characteristics more strongly to its own than to the opposite sex.

(*c*) Curiously enough, the sire's parents seem to have more influence than the dam's. In particular the dam's sire has, within the probable error of our determinations, no influence at all. In the unchanging line of descent, the dam's dam has more influence than the sire's sire, which is what we should expect from (*a*); but (*a*) also makes the male element of much less importance than the female, and so the dam's sire insignificant as compared with the sire's dam. The final result is thus to give a slight preponderance to the sire's over the dam's parents.

(2) *Collateral Inheritance.*

(*a*) The degree of resemblance between puppies of the same parents is not greater when they are of the same than when they are of different litters.

It is clear, however, that we have only been able to find comparatively few pairs of whole siblings from different litters, and the difference between 0·5084 and 0·5257 is of the order of the probable error of the differences. With greater numbers, possibly a more sensible difference might be found for the correlation of siblings from the same and different litters. At present there seems nothing to warrant the idea that puppies from the same litter have the high degree of resemblance which we find between twins in the case of mankind.

(*b*) A comparison of the correlations for half siblings on the dam's side and on the sire's side again emphasises, if the breeding be straightforward, the great prepotency of the dam in the matter of coat-colour. The fact that we have upwards of five times as many pairs of half siblings on the sire's side as on the dam's side shows how large a fashion there is in selecting sires. It is possible that largely used and

* 'Roy. Soc. Proc.,' vol. 61, p. 404. The Table II, p. 410, requires interchange of headings, as already pointed out by Mr. Galton.

possibly overworked sires lose some of their hereditary influence, while not losing their power of fertilising the dam.*

(*c*) The great reduction in the degeee of fraternal correlation when we turn from whole to half siblings is very remarkable, and is, at any rate for half siblings on the dam's side, not very explicable.

Had we assumed the parental correlation to be 0·3507, and found γ from (xxvii), *i.e.* = 1·4722, we should have deduced from (xxviii) for the fraternal correlation the value 0·5236, which is in fair accordance with the observed result for whole siblings. But, as we have seen, (xxvii) and (xxviii) belong to a theory which gives very poor values for the grandparental and great-grandparental correlations, *i.e.*, 0·1753 and 0·0877, instead of 0·1326 and 0·0402. Further, we should on that theory have expected the average correlation for half siblings to be half the value above, since one-half of the common ancestry is cut off, *i.e.*, 0·2618, and not 0·1646, as it actually is. Thus the fraternal correlation does not appear to be in accord with the theory of blended inheritance. Its determination in the general case of exclusive inheritance *with reversion* seems a problem of considerable difficulty, which in this case is rendered much greater by the immense prepotency of the dam, so that it would seem very desirable to differentiate the sexes when dealing with the resemblance of siblings reverting to ancestral types.

(9) *Application of the Theory of Reversion to Basset Hounds.*—We have for mean values $\rho_1 = 0·3507$, $\rho_2 = 0·1326$, $\rho_3 = 0·0404$. Now these correlations certainly do not obey the relation $\rho_1 = 2\rho_2$, $\rho_2 = 2\rho_3$ required, when we take (xxiii) to govern the law of ancestral heredity (*cf.* § 6 (iii)). A glance at the table on p. 149 will show that such a series of ρ's as the above cannot fit into it. Still less do they appear consonant (except to the first roughest approximation) with Mr. Galton's form of that law, *i.e.*, $\gamma = 1$. Nor do they satisfy for the same reasons the law of reversion when we start the reversion series from the parents, *i.e.*, put $\beta = \gamma$ as in § 7 (ii).

Accordingly, I have tried to find what would be the value of ρ_3, if ρ_1 and ρ_2 had the values given above, and our generalised law of reversion were correct. Turning to § 3, and substituting in (xviii) and (xix) for ρ_1 and ρ_2 we have:

$$\delta = 0·286336, \qquad c = 0·698761,$$

* There is also to be considered the possibility of error of the record in the sire's case. Given a large stud of hounds and servants of average carelessness, and a bitch may easily go astray even after she is lined by the dog required. Sir Everett Millais, in a lecture on Telegony, delivered at St. Thomas' Hospital, in 1895, stated that he knew of "quite two dozen such examples resulting in supposed telegony." "The master is the last person to whom such little lapses of duty are confided." But if two dozen mésalliances can be palmed off as telegony, how many alliances within the blood may be conveniently overlooked?

Equation (xx) becomes

$$x^2 - 1{\cdot}56851\,x + 0{\cdot}5 = 0$$

or,

$$\alpha = 0{\cdot}445057.$$

Hence $\gamma = 0{\cdot}019594$, $\beta = 0{\cdot}341279$.

This effectually shows us that for this case γ cannot be taken equal to β, or the reversion series started from the parents.

Further, we reach from Equation (xi):

$$\rho_n = 0{\cdot}30124(\tfrac{1}{2})^n + 0{\cdot}698761(0{\cdot}286336)^n.$$

Such a value again demonstrates that in this case the ancestral correlation differs totally in form from what might be deduced from the theory of blended inheritance, *i.e.*, it shows us how we must distinguish between a law of regression and a law of reversion.

Putting $n = 3$, we have $\rho_3 = 0{\cdot}0541$ for the calculated value of the great grandparental correlation. This may be, I think, considered in satisfactory agreement with the observed value 0·0404. Had we determined our δ and c by the method of least squares, so as to satisfy the three relations

$$\rho_1 = \tfrac{1}{2}(1-c) + c\delta, \qquad \rho_2 = \tfrac{1}{4}(1-c) + c\delta^2, \qquad \rho_3 = \tfrac{1}{8}(1-c) + c\delta^3$$

as closely as possible, we should have got, of course, still more accordant results.

We can now put down our general conclusions:

63·256 per cent. of Basset Hounds	take after their parents	(50)
3·488 „ „	revert to grandparents	(25)
3·105 „ „	„ great grandparents	(12·25)
2·764 „ „	„ great[2] „	(6·25)
2·460 „ „	„ great[3] „	(3·125)
2·190 „ „	„ great[4] „	(1·5625)
1·949 „ „	„ great[5] „	(0·78125)
1·735 „ „	„ great[6] „	(0·390625)

14·053 per cent. of Basset Hounds revert to still higher ancestry (0·390625). Now the divergence here from Mr. Galton's original statement of the law is most significant; I have put in brackets the percentages deduced from that statement. In our case we have a comparatively small reversion to each generation of ancestry, but the percentage, 1·7, is still sensible in the case of the 8th ascending generation. In Mr. Galton's case we have very substantial reversions to grandparents and great grandparents, but the rate of diminution instead of being the loss of about 1/9 at each stage is 1/2! As a result, the reversion to the 8th ascending generation is less than 0·4. It can-

not be denied that the difference is of extreme biological interest. In the former case we have a comparatively small total reversion widely spread; in the latter a much larger total reversion concentrated on the first stages of ancestry. Which system is more accordant with facts? It needs far wider observation and experiment than are yet available to settle this. So far we can only say that the former case covers Mr. Galton's as a special sub-case, and that the data for Basset Hounds appear capable of treatment under the wider rule, but can only be fitted with some straining to the special case. Are there or are there not physiological reasons for supposing that resemblance to a parent arises from a different source from reversion to an ancestor? Here reversion to an ancestor must not be measured by cases of resemblance to an ancestor, for a portion of this resemblance is due to common likeness to the parent; we must approach the matter from the standpoint of cases in which the offspring inherits a character like that of the grandparent or higher ancestor, and *unlike* that of the parent.* Current use of the term "reversion" at least justifies us for the time being in not speaking of all inheritance, including likeness to parents as reversion, and in our theory we may be permitted to differentiate parental and reversional inheritance of exclusive characters, if we find it needed by our numerical data. Here, as elsewhere, we sadly need a widely extended range of observation and experiment.

(10) *On the Variability of Basset Hounds having regard to Sex and Pedigree.*—We have already indicated that our justification in applying the methods of normal correlation to coat-colour is considered in another memoir. We merely suppose at present that there is some variable, following approximately the law of normal variation, on which the coat-colour can be thrown back. This being so, let h in terms of this variable be the distance of its mean value from the division line between tricolour and non-tricolour, and let σ be the standard deviation of this variable for the same group. Then if $\chi = h/\sigma$, we easily deduce by aid of tables of the probability integral from the correlation tables in § 8 the following results:—

* Of C and D brown-eyed parents, A and B, the offspring, have respectively brown and blue eyes. Is the process by which B gets the blue eyes of his grandfather or great grandfather qualitatively different from that by which A gets the brown eyes of his parents? The problem can hardly be answered at present, but I see no *à priori* reason for a negative.

Table of Values of χ.

	χ.		χ.
Sire	1·0806	with all his offspring	0·3891
„	1·0782	„ his ♂ „	0·4242
„	1·1522	„ „ ♀ „	0·3611
Dam	0·1575	„ all her offspring	0·3954
„	0·1315	„ her ♂ „	0·3521
„	0·1422	„ „ ♀ „	0·3186
Sire's sire	1·3372	„ all his offspring	0·4808
„ dam	0·4023	„ „ her „	0·4790
Dam's sire	1·1536	„ „ his „	0·4909
Dam's dam	0·1134	„ „ her „	0·4790
Great grandparents	0·5824	„ all their offspring	0·6592
Whole siblings, same litters			0·3108
„ „ different litters			0·2980
Half siblings, sire's side			0·3143
„ „ dam's side			0·3190

Now, if there were no secular change due to artificial or other selection, h would remain the same in each generation, and therefore $\sigma = h/\chi$ would give a proper measure of the variability due to sex or to relative position in ancestry. The table at once suggests a number of interesting points, which I proceed to note.

(a) Turning first to the offspring, as given in the second column, we observe that the dogs with the longest pedigree have the largest χ. We have, in fact, the values of $1/\chi$ for pedigrees stretching to great grandparents, grandparents, parents, and merely to brethren, respectively, as

$$1·517 : 2·064 : 2·550 : 3·221,$$

or, roughly, we have a geometrical series with the ratio of about 1·3.

Now this result may be reached in more than one way, either (i) decreasing h, or (ii) increasing σ. Increase of h would signify that, for the longer pedigree, the mean of the quantity on which the coat-colour depends is being thrust further into the tricolour section; decrease of σ would signify greater concentration in the tricolour section within which the mean lies. Whether the longer pedigree signifies the more modern hounds, or the more careful preservation of ancestors' names in the more fashionable hounds, we reach practically the same conclusion: the process of breeding is emphasising melanism. Further, by comparing the ♂ and ♀ offspring in the case of both sire and dam, we conclude that this process is sensibly more significant for the male than the female offspring. As very often only one or two puppies out of a litter are recorded in the 'Stud Book,' this apparently artificial selec-

tion, which is more stringent for the males than the females, appears to be, theoretically at least, a mistake when we remember that the potency of the female is thrice that of the male in coat-colour.

Within the groups of grandoffspring and siblings the differences are hardly significant enough for special conclusions to be drawn.

(*b*) Turning next to the parental groups, we see that (i) the sires and dams, neither of which can be considered to form a more modern group than the other, have yet remarkably different values of χ, that for the sire being about seven times as great as that for the dam. The sires are thus far more stringently selected than the dams, and a great deal of this difference must undoubtedly be due to the lesser variability of the sires. Here again the breeders, if they are selecting at all for coat-colour, would appear, at least theoretically, to be in error. (ii) Grandsires, ♂ and ♀, appear to be less variable than the sires, and granddams, ♂ and ♀, less variable than the dams. This may be due to the original paucity of the breed, or be an instance of the general rule to which I have elsewhere referred, *i.e.*, that parents are a selection out of the general population, and so less variable than their offspring.

(*c*) But this rule meets with a remarkable exception in the case of ♀ parentage; both granddams and dams are more variable than their offspring, and very significantly so. An examination of dam and female offspring shows that the ♀ offspring have a value of χ double as great as that of their dams. With few original dams, it is difficult to understand how they could be more variable than their offspring. Considering the great prepotency of the dam, it is difficult to attribute this increase of χ entirely to the action of the less variable sire; one is more or less forced to believe that there is a process of stringent selection of the offspring which are entered on the record going on, and that thus a group of dams possibly fairly variable, and with a not very marked tendency to melanism, is represented in the next generation by offspring of a more stringently selected character; the stringent selection of sires may have contributed, but can hardly be the sole source of this change.

A further conclusion is worth noting: Parents, whether male or female, when they have male are apparently more variable than when they have female offspring.

(10) *General Results.*

(*a*) The laws hitherto propounded for blended inheritance do not appear to cover the cases of exclusive inheritance, *e.g.*, such cases as eye-colour in man, coat-colour in horses or hounds, &c.

(*b*) The law of ancestral heredity must be distinguished from a law of reversion. Neither seem to fit the facts if we adopt the amounts of heritage, $\frac{1}{4}$, $\frac{1}{16}$, $\frac{1}{32}$, &c., from parent, grandparent, great grandparent, &c., originally taken as a first approximation by Mr. Galton.

(*c*) That the mean correlation of an *n*th parent with the offspring is one-half that of an $(n-1)$th parent also appears doubtful. (This would follow if reversion were started from the parent.)

(*d*) Testing theory by the case of Basset Hounds, we find much difficulty, owing partly to the great prepotency of the dam, and partly to the large amount of artificial selection which is evidenced at every turn, and obscures what may be termed the *natural* laws of inheritance.

(*e*) There is an urgent need to widely extend our knowledge of heredity by new experiments and observations on other organs in different races. Facts are of the first necessity at the present time, and facts collected on a large scale for a wide range.*

* It may be of service to indicate to would-be investigators what has already been done or is now in hand:—

In man:—

(*a*) Stature (direct to first degree and collateral, fraternal).
(*b*) Head index (,, ,, ,, ,,).
(*c*) Span and forearm (direct to first degree and collateral, fraternal).
(*d*) Eye-colour (direct to second degree, collateral, fraternal and avuncular).
(*e*) Shape of head, physique, intellectual capacities, tastes (collateral only).
(*f*) Fertility (direct to second degree).
(*g*) Longevity (direct and collateral, fraternal).

In horses:—

(*h*) Coat-colour (direct to second degree and collateral).
(*i*) Fecundity (direct to second degree and collateral, fraternal and avuncular).

In hounds:—

(*j*) Coat-colour (direct to third degree and collateral).

In moths:—

(*k*) Wing-markings (direct and collateral).

In daphnia:—

(*l*) Shape of spine (direct and collateral).

In all these cases the coefficients of correlation have already been worked out, or material is being collected to determine them, by Mr. Francis Galton, Professor W. F. R. Weldon, Dr. Warren, or by my collaborators and myself at University College. Hence I would impress upon others to take as far as possible widely different characters in widely different races. Above all, cases in which artificial selection plays a great part, *i.e.*, dogs, fancy pigeons, &c., ought to be avoided.

February 1, 1900.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "A Case of Monochromatic Vision." By Sir W. DE W. ABNEY, K.C.B., F.R.S.

II. "Thermal Radiation in Absolute Measure." By Dr. J. T. BOTTOMLEY, F.R.S., and Dr. J. C. BEATTIE.

III. "Electrical Conductivity in Gases traversed by Cathode Rays." By J. C. MCLENNAN. Communicated by Professor J. J. THOMSON, F.R.S.

IV. "Researches on Modern Explosives. Second Communication." By W. MACNAB and E. RISTORI. Communicated by Professor RAMSAY, F.R.S.

V. "On the Influence of the Temperature of Liquid Air on Bacteria." By Dr. ALLAN MACFADYEN. Communicated by LORD LISTER, P.R.S.

"On the Effects of Strain on the Thermo-electric Qualities of Metals. Part II." By MAGNUS MACLEAN, M.A., D.Sc. Communicated by LORD KELVIN, G.C.V.O., F.R.S. Received November 22, 1899,—Read January 25, 1900.

A.—*Thermo-electric difference between free wires and wires previously subjected to longitudinal extension and lateral compression, by drawing them through the holes of a draw-plate* (§§ 1—7).

§ 1. In Part I of this paper, read to the Society on 2nd February, 1899, the object of the experiments was stated to be the determination of the *magnitude* of the thermo-electric effects, obtained from any one metal strained and unstrained. The results then given were obtained from two wires of the same material, one wire being previously drawn through a draw-plate, so as to reduce it in size from No. 18 standard gauge (0·122 cm. diameter) to about No. 24 standard gauge (0·0559 cm. diameter). The arrangement of the experi-

ments to measure the thermo-electric effect is shown diagrammatically in fig. 1. One junction of the wires was kept in a glycerine bath which could be heated by a Bunsen burner. This junction was tied by a fine copper wire to the bulb of a thermometer T. The other ends of the wires were joined to short copper wires, which served as terminals of the low resistance galvanometer used in the experiments. These junctions were wrapped in paraffin paper or cotton wool, which contained the bulb of a thermometer T′ reading half degrees from 0° C. to 25° C. A paper screen S was hanging vertically between the Bunsen burner and the thermometer T′ and the galvanometer, to prevent any heat from the flame reaching the rest of the circuit by radiation. These precautions were taken to make certain that all junctions, except the hot junction, would be at the same temperature. The sensitiveness of the galvanometer was 0·09 mikroampere per division, and as its resistance was 1·5 ohms, the electromotive force at its terminals was 0·135 mikrovolt per scale division.

§ 2. The metals for which results were given in Part I were copper (six specimens), lead (two specimens), platinoid, german silver, reostene, and manganin.*

The present paper gives the results of similar experiments made on specimens of commercial† and pure lead, obtained from Messrs. Johnson and Matthey; and specimens of annealed steel, of aluminium and of nickel.

§ 3. The method of experimenting was to take a piece of the wire and draw it through a few holes of a draw-plate, so as to reduce its cross sectional area to about a quarter. Then two pieces of the wire, one drawn and one undrawn, each 60 cm. long, were joined as in fig. 1. The glycerine bath was very slowly heated by the Bunsen burner. When there was a rise of temperature of 5° C. the Bunsen burner was drawn slightly aside, so as to give as much heat to the glycerine bath as it lost by radiation. When both the thermometer reading and the spot of light on the scale were seen to be steady, the readings were noted. The circuit was then broken and readings taken of the galvanometer zero and the thermometer T′. The circuit was again com-

* Dr. Anderson, Chemical Laboratory, the University, Glasgow, gave me the following analyses for reostene and for manganin:—

Reostene.

Si	0·61	per cent.
Fe	79·95	”
Ni	16·53	”
Mn	1·21	”
Total	98·30	

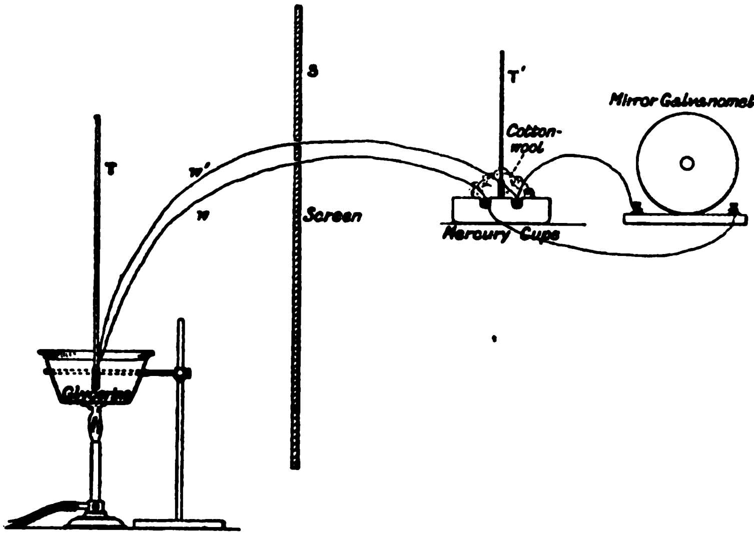
Manganin.

Sn	0·073	per cent.
Fe	0·6	”
Cu	86·62	”
Mn	8·031	”
Ni	3·261	”
Total	98·585	

† Dr. Anderson analysed the commercial lead and found it contained 99·12 per cent. of lead.

pleted, and readings taken when there was 5° C. further rise of temperature, and so on up to a difference of about 100° C., the greatest difference tried in these experiments. A curve was plotted with

Fig. 1.



differences of temperature between the hot and the cold junction as abscissæ, and currents through the galvanometer as ordinates. The mean current per degree difference of temperature as found from each curve is given in Table I.

Table I.

Conductor.	Condition of conductor. Current from 1 to 2 through the hot-junction.	Current in mikro-ampere per degree up to 100° C.
Annealed steel.........	1. Drawn............ 2. Undrawn..........	0·0567
Aluminium............	1. Drawn............ 2. Undrawn..........	0·0065
Nickel................	1. Drawn............ 2. Undrawn..........	0·218
Lead, commercial	1. Undrawn.......... 2. Drawn............	0·0124
Lead, pure	1. Undrawn.......... 2. Drawn............	0·0036

§ 4. The steel wire was annealed by coiling it round a large cast-iron ball, which was heated in a bright coal fire for about an hour. After being taken out of the fire, the ball, with the iron wire round it, was

allowed to cool slowly for about an hour and a half in the ashes below the fire. It was previously found that heating the wire to red heat by an electric current and allowing it to cool slowly did not anneal it.

§ 5. The resistances of all the undrawn wires, and the specific gravities of both the undrawn and drawn wires, were carefully determined by the usual laboratory methods. The results obtained are given in Table II.

Table II.

Conductor.	Cross section of undrawn and drawn wires in sq. cm.	Specific gravity of undrawn and drawn wires.	Resistance of the undrawn wires in C.G.S. units at 14° C.	
			Per c.c.	Per cm. long, weighing a gramme.
Steel, annealed	0·007504 0·004141	7·78 7·762	13,900	108,100
Aluminium	0·04594 0·01697	2·8 2·796	3,546	9,931
Nickel	0·01179 0·002475	8·9 8·85	9,430	83,920
Lead, commercial	0·01145 0·00256	11·36 11·36	20,560	233,100
Lead, pure	0·01150 0·00236	11·35 11·357	20,300	230,200

§ 6. By multiplying the current per division given in Table I, by the total resistance in the circuit, calculated from the results in Table II, the thermo-electric difference per degree between drawn and undrawn wires is found. The numbers are given in Table III.

Table III.

Conductor.	Resistance in ohms of 60 cms. of wire.		Total resistance external to galvanometer.	Total resistance in circuit.	Thermo-electric difference in mikro-volt per degree C. difference of temperature.
	Undrawn.	Drawn.			
Steel, annealed	0·1111	0·2015	0·3126	1·813	0·1028
Aluminium	0·0047	0·0125	0·0172	1·517	0·0099
Nickel	0·0480	0·2287	0·2767	1·777	0·3784
Lead, commercial	0·1077	0·4820	0·5897	2·09	0·026
Lead, pure	0·1059	0·5162	0·6221	2·12	0·0076

§ 7. The thermo-electric difference between glass hard tempered steel, annealed steel, and unannealed steel, was found by similar experiments to be :—

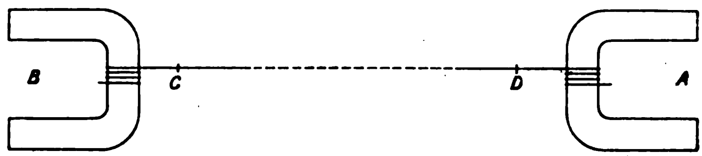
1. Glass hard steel 2. Unannealed steel	} 7·5 mikrovolts per degree C.	
1. Unannealed steel 2. Annealed steel...	} 0·18	" "
1. Glass hard steel 2. Annealed steel...	} 7·67	" "

The direction of the current through the hot junction was in every case from hard steel to soft steel.

B.—*Thermo-electric difference between free wires and wires previously permanently elongated by longitudinal stresses* (§§ 8—10).

§ 8. Attempts were now made to determine the thermo-electric difference between free wires and wires previously permanently elongated by a longitudinal stress. It was found difficult to elongate the hard wires permanently to any appreciable extent before they broke. Several methods for stretching the wires were tried, and the method finally adopted, was to take two pieces of stout copper rod, bent into the shapes shown at A and B in fig. 2, and to wind the wire to be

Fig. 2.



stretched several times round A and B. The end A was clamped in a fixed vice and the end B fixed in the clamp of a screw arrangement. By turning the screw the wire was stretched tight. Two ink marks were then put on the wire at C and D 60 cm. apart. The screw was very slowly turned, and the distance between C and D measured until the necessary elongation was produced or until the wire broke. The wire generally broke where it lay tangentially to either rod A or B.

§ 9. The greatest percentage permanent elongation that could by this method be got in hard drawn copper, manganin, nickel, and German silver, was 0·7, 0·5, 0·7, and 0·5 respectively. The thermo-electric difference between the stretched and the unstretched wires was then determined, as described in § 3, and the results are given in Table IV.

§ 10. It will be noticed that the current is from unstretched to stretched, through the hot junction for three specimens of copper, and from stretched to unstretched through the hot junction for other three specimens. The probable explanation of these results is suggested in §§ 13, 14, 15.

Table IV.

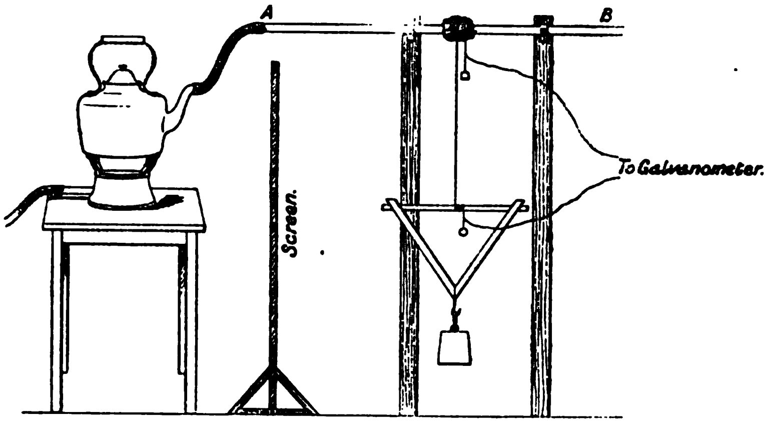
Conductor.	Condition of conductor. Current from 1 to 2 through the hot junction.	Percentage permanent elongation.	Current in mikroampere per degree up to 100° C.	Total resistance in circuit in ohms.	Thermo-electric difference in mikrovolt per degree centigrade difference of temperature.
Messrs. Johnson and Matthey:—					
(*a*) Copper, electrotype	1 stretched 2 unstretched	1·0	0·0021	1·517	0·0032
(*b*) Copper, for alloy	1 stretched 2 unstretched	1·0	0·003	1·519	0·0046
(*c*) Copper, commercial	1 stretched 2 unstretched	1·5	0·0051	1·548	0·0079
Messrs. Glover:—					
(*a*) Copper, hard	1 unstretched 2 stretched	0·7	0·0018	1·519	0·0027
(*b*) Copper, soft	1 unstretched 2 stretched	1·5	0·005	1·531	0·0077
Copper, laboratory	1 unstretched 2 stretched	1·5	0·0024	1·518	0·0036
"	1 unstretched 2 stretched	20·0	0·0174	1·52	0·0264
Reostene	1 unstretched 2 stretched	2·0	0·009	2·312	0·0208
Platinoid	1 stretched 2 unstretched	1·0	0·047	1·937	0·0910
German silver ..	1 stretched 2 unstretched	0·5	0·056	1·835	0·1027
Manganin......	1 stretched 2 unstretched	0·5	0·036	1·924	0·0693
Aluminium	1 stretched 2 unstretched	1·0	0·0135	1·51	0·0204
Nickel	1 unstretched 2 stretched	0·7	0·084	1·596	0·1341

C.—*Thermo-electric difference between free wires and wires under stress, producing* (1) *temporary elongation,* (2) *permanent elongation* (§§ 11—18).

§ 11. The arrangements shown diagrammatically in fig. 3 were now made to determine the thermo-electric difference between free wires and wires while (*a*) under stress, stretching them within their limits of elasticity, and (*b*) under stress, stretching them beyond their limits of elas-

ticity. AB is a brass tube through which steam from a kettle is allowed to pass. The wire under test is wound round this brass tube three times, and then round a small brass tube in the triangular frame below, and then to one of the terminals of the galvanometer. The wire is thus quite continuous, from one terminal of the galvanometer to the other terminal. Some cotton wool is loosely packed at the hot junction of the wire to ensure that the temperature of the wire is at steam temperature. The weight of the triangular frame (two sides wood and

FIG. 3.



one side brass tube) with its hook for hanging weights on was 220 grammes. This is the smallest weight used for each wire, every one of which was about No. 30 S.W.G. (diameter, 0·0315 cm.). The object of the small brass tube in the triangular frame was to keep the temperature of the cold junction at any determined temperature by allowing water or other fluid to flow through it. In all the experiments hitherto made, the temperature of the cold junction was taken as the temperature of the air at the time of each observation.

§ 12. The experiments were performed as follows:—The wire was put into the circuit, as shown in fig. 3. After steam was allowed to pass through the tube for some time, the galvanometer reading and the air temperature were taken. The circuit was then broken, and the metallic zero of the galvanometer was noted. The circuit was made, and a weight was added on to the hook of the triangular frame. Three readings of the galvanometer were now taken: (1) with the weight on, (2) with the weight off, and (3) with the circuit broken. A heavier weight was hung on, and other three readings taken, and so on to the heaviest weight used in the experiments.

§ 13. The readings of the galvanometer were in the same direction

for all the wires tried with weights on and off, except for soft copper and iron. The greatest permanent elongation produced in any of the hard copper wires experimented on was 0·17 per cent., and for this permanent elongation the reading on the galvanometer was in the same direction for weights off and on, though always greater for the latter.

§ 14. For the soft copper wire (Table IX below) the readings were in the same direction for weights on and off, up to a permanent elongation of 1 per cent. After a permanent elongation of 4·72 per cent., the current with weight on was 0·00103 mikroampere per degree from *stretched* to *unstretched* through the hot junction, while with the weight off, the current was 0·00075 mikroampere per degree from *unstretched* to *stretched* through the hot junction.

For iron wire the current was in the same direction for weights on and off, up to a permanent elongation of 0·35 per cent.; but after a permanent elongation of 3·41 per cent. the current with weight on was 0·00461 mikroampere per degree from *unstretched* to *stretched* through the hot junction, and with weight off, 0·0069 mikroampere per degree from *stretched* to *unstretched* through the hot junction.

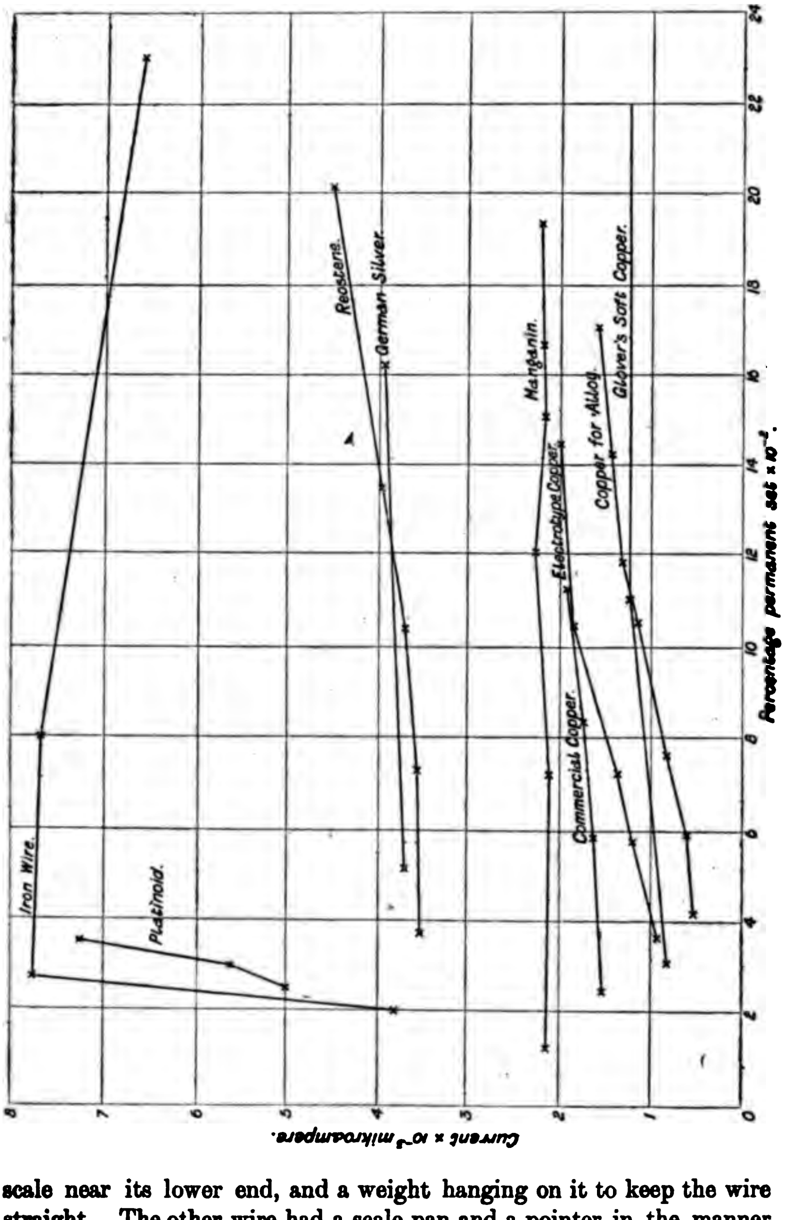
§ 15. In 'Mathematical and Physical Papers,' vol. 2, p. 270, § 109, Kelvin says:—"I have thus arrived at the remarkable conclusion tha when a permanent elongation is left after the withdrawal of a longitudinal force which has been applied to an iron or copper wire, the residual thermo-electric effect is the reverse of the thermo-electric effect which is induced by the force, and which subsists as long as the force acts."

It seems (1) that for small longitudinal strain in copper or in iron the direction of the current through the hot junction is the same, whether the force which produced the permanent strain is on or off, (2) that as the permanent elongation is increased by increased longitudinal forces, a stage is reached which gives zero current when the forces are removed, and (3) that for greater longitudinal forces and permanent elongations the direction of the current is opposite, with the pulling forces off and on. It seems, in fact, that the permanent elongation must exceed a definite limit, to produce reverse thermo-electric effects with the longitudinal force on and removed. I hope to further investigate this point and to report the results to the Society.

§ 16. The galvanometer used for the investigation of these temporary and permanent strains was one of the Kelvin recorder pattern, namely, a movable coil between the poles of a strong permanent magnet of circular form. The coil had 81 turns and a resistance 14·94 ohms at 17°·5 C. Its constant was determined in the usual way, and found to be 0·029 mikroampere per division of the scale.

§ 17. To find the stress-strain diagram, experiments were performed on a specimen of each wire, in the following manner. Two pieces of the wire were passed through two small holes in a metal plate, and

soldered at the back of the plate. This plate was fixed to a horizontal support at a convenient height. One of the wires had a half millimetre



scale near its lower end, and a weight hanging on it to keep the wire straight. The other wire had a scale pan and a pointer in the manner generally used in laboratories for finding the Young's modulus of

materials. By putting weights into the scale pan, and taking them out, and noting the readings of the pointer on the scale, the temporary and permanent elongation in the wire for different weights were found. The numbers are given in the second and third columns of Tables V to XIV. The currents in the fourth columns are calculated from the deflections of the galvanometer when the stated weights are on the wire, and the thermo-electric differences in the fifth column are found by multiplying the currents in the fourth column by the total resistance in the circuit in each case.

§ 18. The numbers in the tables are plotted in curves with percentage permanent elongation as abscissæ, and thermo-electric differences as ordinates. The thermo-electric difference between free platinoid and strained platinoid, rises rapidly with the permanent elongation of the strained wire. For manganin and lead, both commercial and pure, it is very nearly constant up to a permanent elongation of $\frac{1}{8}$ per cent.

Mr. Alexander Wood, Thomson Experimental Scholar in the Physical Laboratory, The University, Glasgow, has rendered valuable help in the experimental work and in the calculations.

Table V.—Electrotype Copper.

Current from Stretched to Unstretched.

Temperature difference = 87° C. Total resistance in circuit = 15·56 ohms.

Total weight in grammes.	Percentage temporary elongation.	Percentage permanent elongation.	Current in mikroampere per degree with weight on.	Thermo-electric difference in mikrovolt per degree.
250	0·06	..	0·00020	0·00311
500	0·1	..	0·000267	0·00415
750	0·137	..	0·000567	0·00882
1000	0·18	0·036	0·000934	0·01452
1250	0·21	0·057	0·001200	0·01867
1500	0·264	0·072	0·00137	0·02127
1750	0·318	0·104	0·001866	0·02905
2000	0·348	0·144	0·002	0·03113

Table VI.—Commercial Copper Wire.

Current from Stretched to Unstretched.

Temperature difference = 88° C. Total resistance in circuit = 16·66 ohms.

Total weight in grammes.	Percentage temporary elongation.	Percentage permanent elongation.	Current in mikroampere per degree with weight on.	Thermo-electric difference in mikrovolt per degree.
250	0·05	..	0·00119	0·02
500	0·1	..	0·00142	0·0236
750	0·14	..	0·00138	0·0231
1000	0·19	..	0·00148	0·0247
1250	0·23	0·024	0·00152	0·0253
1500	0·28	0·058	0·00161	0·0269
1750	0·32	0·088	0·00175	0·0291
2000	0·37	0·112	0·00191	0·0318

Table VII.—Copper for Alloy.

Current from Stretched to Unstretched.

Temperature difference = 87° C. Total resistance in circuit = 15·62 ohms.

Total weight in grammes.	Percentage temporary elongation.	Percentage permanent elongation.	Current in mikroampere per degree with weight on.	Thermo-electric difference in mikrovolt per degree.
250	0·08	..	0·000333	0·00521
500	0·145	0·041	0·000533	0·00833
750	0·176	0·059	0·000600	0·00937
1000	0·215	0·076	0·000833	0·01302
1250	0·256	0·105	0·001167	0·01823
1500	0·285	0·118	0·001324	0·02031
1750	0·340	0·142	0·001433	0·02239
2000	0·402	0·170	0·001600	0·02500

Table VIII.—Glover's Hard Copper.

Current from Stretched to Unstretched.

Temperature difference = 86°·5 C. Total resistance in circuit = 15·59 ohms.

Total weight in grammes.	Percentage temporary elongation.	Percentage permanent elongation.	Current in mikroampere per degree with weight on.	Thermo-electric difference in mikrovolt per degree.
250	..	..	0·000335	0·005226
500	0·006	..	0·000469	0·007316
750	0·014	..	0·001106	0·01725
1000	0·019	0·006	0·001241	0·01934
1250	0·0285	0·0135	0·001375	0·02143

Table IX.—Glover's Soft Copper.

Current from Stretched to Unstretched.

Temperature difference = 87° C. Total resistance in circuit = 15·56 ohms.

Total weight in grammes.	Percentage temporary elongation.	Percentage permanent elongation.	Current in mikroampere per degree with weight on.	Thermo-electric difference in mikrovolt per degree.
300	..	..	0·00020	0·003113
500	0·14	0·03	0·00083	0·01296
750	0·28	0·105	0·00123	0·01919
1000	1·18	1·00	0·00143	0·02225
1200	4·84	4·72	0·00103	0·01608

Table X.—Reostene Wire.

Current from Unstretched to Stretched.

Temperature difference = 86°·5 C. Total resistance in circuit = 43·39 ohms.

Total weight in grammes.	Percentage temporary elongation.	Percentage permanent elongation.	Current in mikroampere per degree with weight on.	Thermo-electric difference in mikrovolt per degree.
250	0·037	..	0·002682	0·1163
500	0·085	..	0·003521	0·1528
750	0·152	0·037	0·003521	0·1528
1000	0·220	0·073	0·003588	0·1557
1250	0·257	0·104	0·003721	0·1614
1500	0·318	0·134	0·003957	0·1717
1750	0·417	0·201	0·004526	0·1963

Table XI.—Platinoid Wire.

Current from Stretched to Unstretched.

Temperature difference = 86° C. Total resistance in circuit = 29·89 ohms.

Total weight in grammes.	Percentage temporary elongation.	Percentage permanent elongation.	Current in mikroampere per degree with weight on.	Thermo-electric difference in mikrovolt per degree.
250	0·035	..	0·003541	0·1058
500	0·071	..	0·003979	0·1190
750	0·101	..	0·004451	0·1330
1000	0·130	0·025	0·005125	0·1532
1250	0·16	0·03	0·005631	0·1683
1500	0·19	0·035	0·007285	0·2177

Table XII.—German Silver Wire.

Current from Stretched to Unstretched.

Temperature difference = 86° C. Total resistance in circuit = 26·85 ohms.

Total weight in grammes.	Percentage temporary elongation.	Percentage permanent elongation.	Current in mikroampere per degree with weight on.	Thermo-electric difference in mikrovolt per degree.
250	..	..	0·002529	0·0679
500	0·08	..	0·002360	0·0634
750	0·105	..	0·002474	0·0661
1000	0·13	..	0·003020	0·0813
1250	0·18	0·051	0·003710	0·0996
1500	0·35	0·162	0·003946	0·1059
1750	0·80	0·588	0·004552	0·1222

Table XIII.—Manganin Wire.

Current from Stretched to Unstretched.

Temperature difference = 86°. Total resistance in circuit = 30 ohms.

Total weight in grammes.	Percentage temporary elongation.	Percentage permanent elongation.	Current in mikroampere per degree with weight on.	Thermo-electric difference in mikrovolt per degree.
250	..	..	0·001888	0·05665
500	0·145	0·012	0·002124	0·06372
750	0·260	0·072	0·002090	0·06272
1000	0·327	0·120	0·002259	0·06778
1250	0·382	0·151	0·002158	0·06474
1500	0·427	0·166	0·002158	0·06474
1750	0·484	0·193	0·002192	0·06575

Table XIV.—Iron Wire.

Current from Unstretched to Stretched.

Temperature difference = 85°. Total resistance in circuit = 19·20 ohms.

Total weight in grammes.	Percentage temporary elongation.	Percentage permanent elongation.	Current in mikroampere per degree with weight on.	Thermo-electric difference in mikrovolt per degree.
250	..	..	0·0000727	0·000912
500	..	..	0·000181	0·006216
750	0·023	..	0·000131	0·006216
1000	0·03	0·02	0·003813	0·07192
1250	0·06	0·027	0·007816	0·09485
1500	0·19	0·08	0·007736	0·09430
1750	0·39	0·23	0·006602	0·08696
2000	..	3·41	0·00461	0·07576

"A Case of Monochromatic Vision." By Sir W. DE W. ABNEY, K.C.B., F.R.S. Received January 17,—Read February 1, 1900.

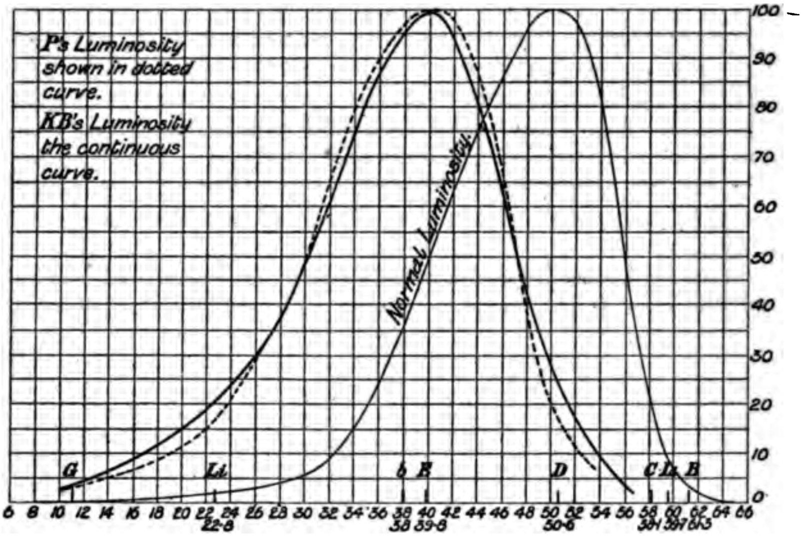
Cases of monochromatic vision are rare, and I have thought it right to put on record one which was kindly brought me some time ago by Mr. Parker. The patient, whom we will call K. B., was aged twenty-five at the time when I examined him for colour vision. The notes of his case are as follows:—Vision always defective; has always been colour blind. Has quick horizontal nystagmus; probably an absolute central scotoma. He is always "day blind." His vision for right and left eyes is 6/60. He is not night blind. His fields are nearly, but not quite, full for white. He shows no definite changes in his eyes.

I took his luminosity curve, and all colours he matched with white with the same facility as if they were white. The following table

Scale of spectrum (prismatic).	K. B.'s luminosity.	P.'s luminosity.	Scale of spectrum (prismatic).	K. B.'s luminosity.	P.'s luminosity.
56	2·5	—	32	61·5	65·0
54	9·0	—	30	43·0	50·0
52	16·0	7·0	28	37·0	36·0
50	27·5	19·0	26	30·0	26·5
48	42·5	39·0	24	24·0	19·5
46	61·0	65·0	22	18·5	14·0
44	82·5	85·0	20	14·5	10·0
42	96·0	98·0	18	11·5	—
40	100·0	99·0	16	9·0	5·5
38	95·5	97·5	14	7·0	—
36	87·5	90·0	12	5·0	—
34	75·0	80·0	10	3·0	2·5

gives the luminosity of the spectrum to him, and for the convenience of reference a previous case, which has already appeared in the 'Proceedings,' is given for comparison. In the accompanying diagram both these curves are shown, together with the curve of luminosity for the normal eye. As regards the first two, it will be seen that the maximum of each curve is about scale number 40, or close to E. On the right-hand side of the maximum the curves do not absolutely agree. K.B.'s observations were first made in the red and green, and his readings at first were not very close, and a mean had to be taken. As the colours he had measured went towards the blue his measures were much more accordant, as he had become accustomed to the methods

employed. The slight divergence on the left-hand side of the curve from that of P is probably due to his colouring matter in the yellow spot. Attention must be again called to the fact that these curves are practically identical with those obtained by the normal eye when it measures a spectrum of very feeble luminosity, and also agree with



the results obtained by measuring the diminution of each ray when it first becomes invisible, and making a curve of the reciprocals of the numbers, taking the highest point of it as 100. This is clearly shown in Part III, "Colour Photometry."* It may be mentioned the scale of the prismatic spectrum employed is the same in this communication as in that paper, the wave-lengths of each scale number being given in it.

"On the Influence of the Temperature of Liquid Air on Bacteria." By ALLAN MACFADYEN, M.D. Communicated by LORD LISTER, Pres. R.S. Received December 15, 1899,—Read February 1, 1900.

The experiments of Dr. Horace T. Brown and Mr. Escombe† have shown that no appreciable influence is exerted upon the germinative power of seeds, when exposed for 110 hours to the temperature of

* 'Phil. Trans.,' A, vol. 183, 1892.
† 'Roy. Soc. Proc.,' vol. 62, 1898, p. 160.

liquid air (– 183° C. to – 192° C.). The results were equally negative in the recent experiments of Sir W. Thiselton-Dyer,* in which seeds survived exposure for upwards of six hours to the temperature of liquid hydrogen (– 250° C. to – 252° C.).

The following investigation on the influence of the temperature of liquid air on bacteria, was carried out at the suggestion of Sir James Crichton Browne and Professor Dewar. The necessary facilities were most kindly given at the Royal Institution. The experiments were conducted under the personal supervision of Professor Dewar, and he has asked me to put the results on record, although it must be acknowledged that the essential features of the investigation are due to him.

The bacteria employed were selected from the stock of the Jenner Institute of Preventive Medicine, where the results were also controlled. Pure cultures of the several micro-organisms were employed, and the series included typical representatives of saprophytic and parasitic bacteria. The organisms chosen possessed varying degrees of resistance to external agents—the extremes in this respect being represented by the very sensitive spirillum of *Cholera Asiatica* and the highly resistant spores of *B. anthracis.*

Ten organisms were used for the experiments, viz. :—*B. typhosus, B. coli communis, B. diphtheriæ, Spirillum choleræ Asiaticæ, B. proteus vulgaris, B. acidi lactici, B. anthracis* (sporing culture), *Staphylococcus pyogenes aureus, B. phosphorescens* and *Photobacterium balticum.*

The cultures of the organisms were young and vigorous, and were tested both on solid and in fluid media, viz. :—Nutrient gelatin, agar-agar, potato and peptone broth.

The cultures on these media were simultaneously exposed to the temperature of liquid air for twenty hours (– 182° C. to – 190° C.). They were then carefully thawed and examined. The results may be briefly stated. In no instance, whether on solid or in liquid media, could any impairment of the vitality of the micro-organisms be detected. The fresh growths obtained from the exposed tubes were normal in every respect, and the functional activities of the bacteria were equally unaffected. The colon bacillus produced its typical effects—such as the curdling of milk, the fermentation of sugar and the production of indol; the *Staphylococcus pyogenes aureus* retained its pigment producing properties and the anthrax spores their pathogenic action on animals. The photogenic bacteria preserved their normal luminous properties. These photogenic properties are intimately connected with the functional activities of the cells. The cells emit light which is apparently produced by a chemical process of intracellular oxidation and the phenomenon ceases with the cessation of their activity. These organisms therefore furnished a very happy

* 'Roy. Soc. Proc.,' vol. 65, 1899, p. 361.

test of the influence of low temperatures on vital phenomena. Their cultures, when cooled down in the liquid air for twenty hours, became non-luminous, but on re-thawing the luminosity returned with unimpaired vigour as the cells renewed their activity. Watery emulsions of the photogenic bacteria, on immersion in liquid air for a few minutes, ceased to emit light, but on withdrawal the luminosity reappeared in a very short time. Strips of filter paper soaked in the watery emulsions and brightly luminous were immersed directly in the liquid air with similar results. The sudden cessation and rapid renewal of the photogenic properties of the cells, despite the extreme changes of temperature, was remarkable and striking.

The following experiment was made:—Fifty litres of the laboratory air about six feet from the ground were liquefied at atmospheric pressure in a glass bulb by means of boiling liquid air *in vacuo*. The temperature reached was about $-210°$ C. The bulb was then sealed off, the contents being still at a temperature below zero, and was subsequently opened and washed out with sterile broth. A series of plate cultures were made from the broth on nutrient gelatin, agar-agar and sugar agar, and were incubated under aërobic and anaërobic conditions at 22° and 37° C. for a period of ten days. The anaërobic plate cultures remained sterile. The aërobic plates yielded forty-four organisms which had survived an exposure to $-210°$ C. The organisms were representative types of those to be usually met with in the air, viz., moulds, bacilli, cocci, torulæ and sarcinæ.

It may also be mentioned that a sample of yeast cell plasma (Buchner's zymase) subjected to $-182°$ C. to $-190°$ C. for twenty hours, retained its peculiar properties unchanged, viz., as regards the production of CO_2 and alcohol.

The above experiments show that bacteria may be cooled down to $-190°$ C. for a period of twenty hours without losing any of their vital properties.

Further experiments are in progress with the above-mentioned and with other micro-organisms, exposed to the temperature of liquid air for still longer periods of time, as well as to that of liquid hydrogen. These experiments will form the subject of a future communication.

February 8, 1900.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Spectrum of α Aquilæ." By Sir NORMAN LOCKYER, K.C.B., F.R.S., and A. FOWLER.

II. "On the Production of Artificial Colour-blindness by Moonlight." By G. J. BURCH. Communicated by Professor GOTCH, F.R.S.

III. "On the Relation of Artificial Colour-blindness to Successive Contrast." By G. J. BURCH. Communicated by Professor GOTCH, F.R.S.

IV. "On Electrical Effects due to Evaporation of Sodium in Air and other Gases." By W. CRAIG HENDERSON. Communicated by LORD KELVIN, F.R.S.

V. "On Electric *Touch* and the Molecular Changes produced in Matter by Electric Waves." By Professor J. CHUNDER BOSE. Communicated by LORD RAYLEIGH, F.R.S.

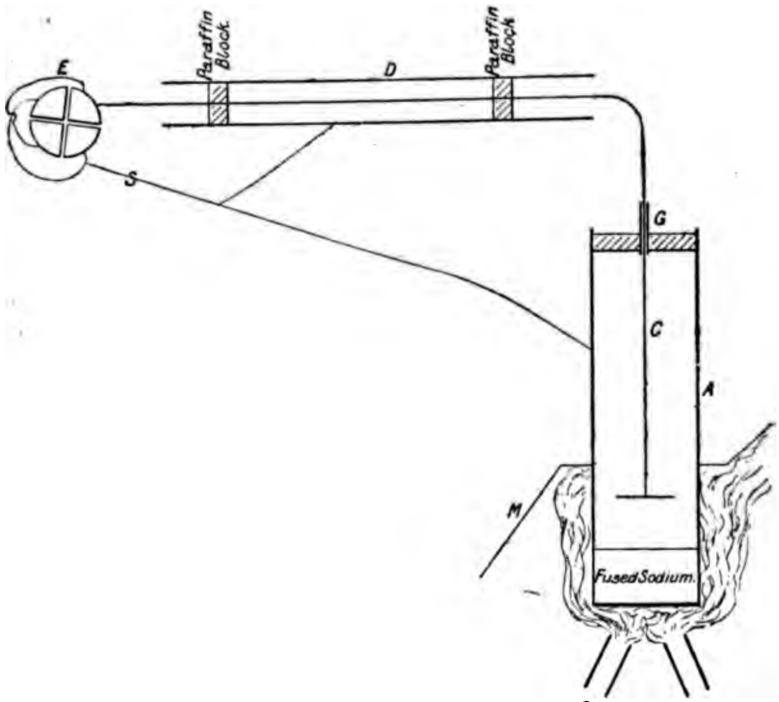
"On Electrical Effects due to Evaporation of Sodium in Air and other Gases." By W. CRAIG HENDERSON, M.A., B.Sc., late 1851 Exhibition Science Scholar. Communicated by LORD KELVIN, F.R.S. Received November 30, 1899,—Read February 8, 1900.

The experiments described below form part of a research which I began in the summer of 1897 in the Cavendish Laboratory, Cambridge, but was unable to complete before leaving Cambridge that same year. The object of this part of the research, which was suggested by Professor J. J. Thomson, was to determine whether evaporation of an unelectrified liquid produces any electrification or not.

The liquid used was fused sodium and the arrangement of apparatus is shown in the accompanying drawing.

The sodium to be fused was held in a vertical iron cylinder, A,

closed at the bottom and having a tightly fitting asbestos plug at the mouth. Through a small hole in the asbestos plug there passed a tightly fitting glass tube, G, only a little longer than the thickness of the plug. A stout bare copper wire, C, passed through this tube G without touching the sides, and had a copper disc at the end inside



the iron cylinder. Outside the cylinder, this wire passed direct to the insulated quadrants of an electrometer E, and was surrounded throughout this portion of its length by a metal guard-tube D, which screened it from outside electrostatic influences. Besides the fastening to the electrometer, the sole supports of the wire C were two paraffin plugs fixed into the ends of this tube D. The iron cylinder A, the tube D, and the uninsulated quadrants of the electrometer E, were connected by a wire with one another and with the sheath of the electrometer, denoted by S in the diagram.

The heat to fuse the sodium was supplied by two Bunsen burners placed below the cylinder A; and in order to protect the insulated wire from the hot gases rising from the burners, a metal screen M was fixed on the cylinder and bent up on the side remote from the electrometer to serve as a funnel.

With these arrangements it was found that very soon after heat was applied to the sodium, a negative electrification of 2 to 3 volts was indicated by the electrometer. This electrification persisted for a considerable time, but eventually the insulation broke down, owing to the sodium vapour condensing in the glass tube G at the mouth of the cylinder, sufficiently to cause a solid connection between the tube and wire.

When the same experiment was repeated *without* the sodium, no electrification was indicated by the electrometer.

These results were confirmed by repeated experiments, and the question then arose whether this negative electrification is due to the evaporation of the sodium, or to oxidation of the sodium going on in the iron cylinder.

The latter seemed probable, as the electrometer showed electrification almost from the moment when heat was applied to the sodium, whereas sodium does not fuse till at temperature of 96° C. and boils at about 400° C.

To determine this point, the same experiment was repeated with this difference, that the air in the cylinder was replaced by an atmosphere in which the sodium could not oxidise. Carbonic acid gas was first tried, being kept flowing into the cylinder after passing through a drying apparatus; but in this gas the sodium became coated with a white encrustation, and showed no signs of evaporation or of boiling even at a red heat.

Coal gas was next tried, and no difficulty was found in boiling the sodium in this gas. To prevent accident by explosion, the apparatus was set up in the fire-place, so that the escaping coal gas might pass up the chimney, and not mix with the air of the room. Great care was taken to ensure the complete removal of air from the iron cylinder before heat was applied. With this atmosphere of coal gas no electrification was obtained while heat was applied to the tube for over an hour. The insulation was tested from time to time during this period by giving a charge to the wire from an electrified vulcanite rod, watching the rate of leak indicated on the electrometer scale, and then discharging. It was found to be excellent; but eventually, as before, it broke down when some of the sodium vapour condensed in the mouth of the cylinder between the wire and the glass tube. Repetition of this experiment confirmed this result.

This problem of the possible generation of electricity by evaporation of a liquid, has been recently investigated for the case of water by Pellat,* who found no trace of electrification. A similar result was found in the earlier experiments of Blake,† who used water and solutions

* Pellat, 'Séances de la Société Française de Physique, 1899,' 1er Fascicule, p. 21.

† Blake, 'Wiedemann's Annalen,' vol. 19, 1883, p. 518.

of copper sulphate and of sodium chloride, but found no electrification. The present experiments lead to the conclusion that evaporation of fused sodium does not give electrification, such as could be detected by the method used, unless oxidation is going on.

February 15, 1900.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Genesis and Development of the Wall and Connecting Threads in the Plant Cell. Preliminary Communication." By WALTER GARDINER, F.R.S.

II. "Photography of Sound-waves and the Kinematographic Demonstration of the Evolutions of Reflected Wave-fronts." By R. W. WOOD. Communicated by C. V. BOYS, F.R.S.

"The Genesis and Development of the Wall and Connecting Threads in the Plant Cell. Preliminary Communication." By WALTER GARDINER, M.A., F.R.S., Fellow and Bursar of Clare College, Cambridge. Received February 1,—Read February 15, 1900.

In the course of my investigations in connection with the forthcoming paper on "The Histology of the Cell Wall with special Reference to the Mode of Connection of Cells,"* certain observations and conclusions concerning the origin and development of the wall-threads and cell-wall have come to light which seem to be of sufficient interest to warrant my bringing them to the notice of the Society without delay.

1. *Origin and Development of the "Wall Connecting Threads."*

The "connecting threads" are found to arise from the median nodes of the fibres of the achromatic spindle. The nodes are either (*a*) all continued as connecting threads, *e.g.*, the endosperm cells of *Tamus*

* 'Roy. Soc. Proc.,' vol. 62, 1897. (Preliminary Communication.)

communis; (*b*) in part continued, and in part overlaid by superposed lamellæ of cellulose membrane, *e.g.*, the endosperm cells of *Lilium Martagon;* or, (*c*) all overlaid, *e.g.*, the pollen mother-cells and pollen grains of *Helleborus fœtidus.*

2. *Origin and Development of the Cell Wall.*

(*a*) *Origin.*—Seeing that spindle fibre nodes (apparently intact) can be recognised in a mature wall of considerable thickness, there would seem little doubt that the existing views with regard to the genesis of the cell plate and first formed cell wall cannot be entirely correct.

I am inclined to believe that the cell plate arises not directly from the spindle fibres, in the manner described by Strasburger and others, but rather indirectly; that is to say, that although it is possibly provided by or even proceeds from the fibres in question, yet it exhibits a certain structural distinction, in that it is pierced by the persistent nodes of the spindle fibres, and is not merged into their substance.

The cell plate would appear to consist of cytoplasm, and cytoplasm, moreover, practically identical with the ordinary cytoplasm of the cell, and from it is secreted the first formed cell wall as an equatorial membrane traversed by the nodes of the achromatin spindle fibres.

(*b*) *Development.*—There are grounds for regarding the primary cell wall as different in genesis and character from the secondary formations which succeed it and arise from the general cytoplasm. In any case, the wall rapidly grows in thickness as layer after layer of cellulose is deposited. In the course of my work certain observations were made, which appeared to throw some light on the structure and genesis of the wall thus produced. It was found that many walls, and especially mucilaginous walls, when strongly swollen and stained, after passing through the stage of stratification, became resolved into numberless and often well-defined spherical droplets or spherules which not unfrequently exhibit a markedly high refraction, and are embedded in a hyaline and possibly mucilaginous ground-substance or matrix.

I am of opinion that these spheroidal droplets represent swollen granules or spherules, which are practically homologous with the droplets or the drops (and I am disposed to think with the droplets) described by myself and Ito in our paper "On the Structure of the Mucilage-secreting Cells of *Blechnum occidentale*, L., and *Osmunda regalis*, L.," published in 1887 in the August number of the 'Annals of Botany'; and I believe that the phenomena in the two cases of internal mucilage there described, were in essence, instances of internal wall formation, or, in other words, that the formation of the cell wall takes place in a similar way. Moreover, the "mucilage" described by us, both gave the reactions of cellulose, and also exhibited the formation of a firm, clear, and *stratified* membrane.

In the above paper we compared the droplets of *Blechnum occidentale* with the granules or spherules described by Langley as occurring in certain gland cells, *e.g.*, the mucous cells of the sub-maxillary gland of the dog; and I am still of opinion that such a comparison was a pertinent one, and not entirely without significance in the case of the plant cell wall also.

I am disposed to regard the cell wall as fundamentally of the nature of a mucous or, rather, mucilage secretion; the droplet or spherules (shall I call them provisionally "*teichosomes*"?) being composed of a substance which, when more hydrated, passes as "a mucilage," and when less hydrated functions as "a cellulose." The spherules are embedded in the "ground substance," and possibly the remains of even a protoplasmic framework (which may undergo mucilaginous change) is also present.

I regard stratification as the necessary accompaniment of the rhythmic periods of activity and rest of the secreting protoplasm; and as to the method of secretion, it is *external* and not internal, as in the mucilage cells described by Gardiner and Ito.

The changes incident upon lignification and the like I have always regarded as induced by secondary secretion or post-formation chemical change.

I may add that I see little in the above view of the structure of the cell wall which militates against the facts which we have at our disposal, either with regard to the properties of the cell wall or to the phenomena associated with growth in thickness or in surface.

I am aware that much remains to be done before the above views are placed on a proper basis, but I have great hopes that this is only a matter of time and of further detailed research.

February 22, 1900.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Preliminary Note on the Spectrum of the Corona. Part 2." By Sir NORMAN LOCKYER, K.C.B., F.R.S.

II. "On the Structure of Coccospheres and the Origin of Coccoliths." By Dr. H. H. DIXON. Communicated by Professor J. JOLY, F.R.S.

III. "The Ionisation of Dilute Solutions at the Freezing Point." By W. C. D. WHETHAM. Communicated by E. H. GRIFFITHS, F.R.S.

"Preliminary Note on the Spectrum of the Corona. Part 2." By Sir NORMAN LOCKYER, K.C.B., F.R.S. Received February 8,—Read February 22, 1900.

One of the chief results which, in my opinion, would be secured by the use of the prismatic camera in eclipse work was the differentiation between chromospheric and coronal phenomena. The photographs taken during the eclipses of 1893, 1896, and 1898 all enabled this distinction to be made very clearly, and various radiations formerly attributed to the corona have been shown to belong to the chromosphere alone. The photographs taken in Africa in 1893 showed eight rings in the spectrum of the corona; in Novaya Zemlaya, in 1896, with a less powerful instrument, a smaller number was secured; but those taken with increased dispersion in India, in 1898, show a much greater number.

I have already given the results of an inquiry into the wave-lengths of two of the chief coronal rings (5303·7 and 4231·3) as determined from photographs taken in 1898 with the 6-inch prismatic camera;* and as the results of the continued investigations may be of service to intending observers of the eclipse of next May, I give a short abstract of them in the present note.

Eight photographs were obtained during the time the spectrum of the corona was least admixed with that of chromosphere; of these, three taken with instantaneous exposures show only two or three of the brighter rings, so that five, showing many coronal rings, are suitable for measurement. The exposures of the photographs used for measurement were as follows, the ratio of focal length to aperture being 15:—

No. 2*a*	50	seconds.
2*c*	6	"
3*a*	12	"
3*c*	7	"
3*d*	8	"

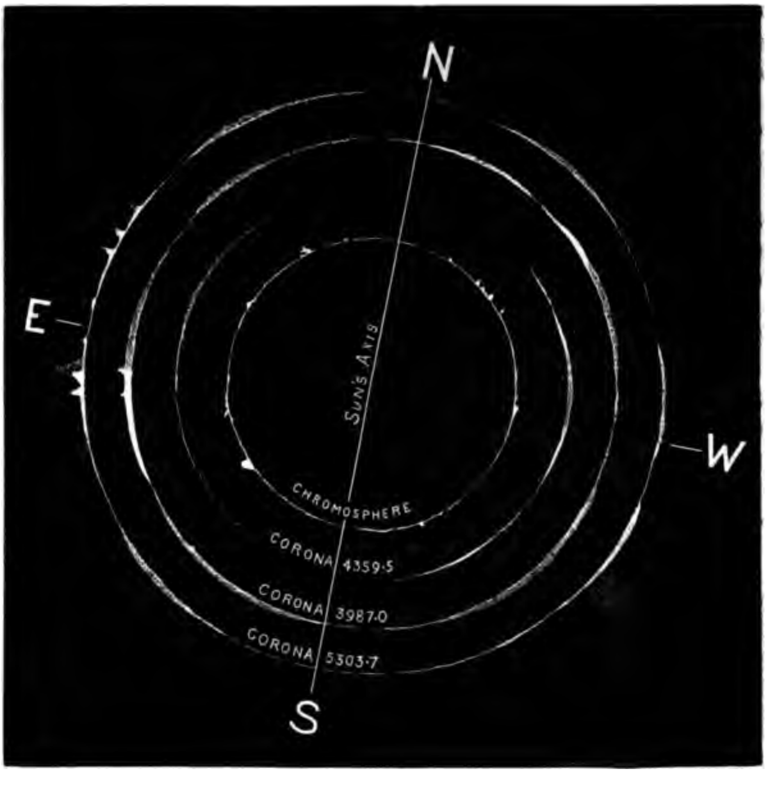
During the earlier part of the exposure of plate 2*a*, the upper regions of the chromosphere were visible in the north-east, and some of the stronger chromospheric arcs appear, together with the coronal rings, in the corresponding parts of the images. In plates 3*c* and 3*d* chromospheric arcs appear in the south-west quadrant, together with

* 'Roy. Soc. Proc.,' vol. 64, p. 168.

coronal rings; but, as I pointed out in the preliminary report on the observations at Viziadrug, the arcs and rings are readily distinguished.*

The coronal rings which have been noted on the photographs may be divided into three groups, defined by the position-angles in which they have their greatest brightness. The typical rings are (1) the green ring at λ 5303·7; (2) a violet ring at λ 3987, near Hϵ; and (3) a blue ring at λ 4359·5, near Hγ. The structure and brightness of these are shown in the accompanying diagram, but it may be remarked that the fainter members of the three groups do not exhibit the differences of structure so clearly.

FIG. 1.—Diagram showing the Forms of Three Typical Coronal Rings, and the Positions of the Prominences photographed at the same time.



The tables which follow show the wave-lengths of the rings which are believed to belong to each of the three groups, and indicate also the average brightness of each ring.

* 'Roy. Soc. Proc.,' vol. 64, p. 38.

Table of Coronal Rings.

Group I. Typical ring, λ 5303·7.

Wave-length.	Brightness. Max. = 10.	Wave-length.	Brightness. Max. = 10.
3952·5	2	4536	1
4007	1	4588·5	1
4022	1	4657	1
4056	2	4685·5	2
4068	1	4714	1
4085	1	4727	1
4121	1	4737	1
4168	1	4768	1
4220	2	4808	1
4231·3	5	4922	2
4248·5	2	5125	1
4262	1	5137	1
4400	1	5303·7	10
4430	1		
4518	1		

Group II. Typical ring, λ 3987·0.

Wave-length.	Brightness. Max. = 10.
3800	3
3987·0	5
4275	1
4568·5	3

Group III. Typical ring, λ 4359·5.

Wave-length.	Brightness. Max. = 10.
4030	1
4192	1
4204	1
4302	1
4323	2
4359·5	3
4485	1
4648	1
4662	1
4788	1
4890	1
5001	1
5255	1

I have already suggested that the different forms of the coronal rings indicate that they are not all due to the same substance, and the foregoing tables suggest that at least three substances are in question. The attempts which have so far been made to trace the origins of the rings, however, have led to no very definite results, and the coincidences with lines in the spectra of stars and nebulæ which were formerly suspected have not yet been completely established.

Special interest is attached to the question of the presence or absence of carbon flutings. There is a possible trace of the fluting,

commencing at λ 4736·18, which so far has not been observed in the chromosphere. The other flutings of carbon which are present in the chromosphere do not appear in the coronal spectrum.

The reductions indicate that there may be feeble indications of the presence of some of the chromospheric gases in the inner corona. Thus in photograph *3d*, on the north-eastern edge, fragments of rings corresponding to lines of helium at λλ 4472, 4714, and 4922 have been recorded; these occur also on the south-western limb, where the chromosphere itself is coming into view, but as the chromosphere was completely eclipsed in the north-east at this stage, the radiations mentioned as occurring there perhaps belong to the inner corona.

A very interesting result of this detailed examination of the photographs is that the chief coronal ring in the green is very closely associated with the form of the inner, and appears to have no distinct connection with the outer, corona. This suggests that the green line of the coronal spectrum is not produced in the outer corona, and that the indications of its presence there on previous occasions, as obtained by slit spectroscopes, were simply due to glare, as in the case of hydrogen and calcium. So far as the photographs taken with the prismatic cameras are concerned, the spectrum of the outer corona gives no indications of bright rings.

The measurements of the coronal rings and the diagram which accompanies this paper have been made by Mr. Fowler.

Dr. Lockyer has investigated the coronal spectrum in relation to carbon, and Mr. Baxandall has made comparisons with the spectra of stars and nebulæ.

"The Ionisation of Dilute Solutions at the Freezing Point." By W. C. D. WHETHAM, M.A., Fellow of Trinity College, Cambridge. Communicated by E. H. GRIFFITHS, F.R.S. Received February 14,—Read February 22, 1900.

(Abstract.)

It is known that the depression of the freezing point of water, produced by dissolving molecularly equivalent amounts of different acids and salts in a given quantity of it, is approximately proportional to the number of ions which these substances must be supposed to yield in order to explain their electrical conductivities. Again, as the concentration of a solution of one such substance is gradually increased, the molecular depression of the freezing point, and the equivalent electrical conductivity, both vary, and vary by amounts which seem in some cases to correspond, but in others to differ considerably.

There appeared reason to suppose that it was desirable to increase

both the extent and the accuracy of our experimental knowledge of these relations. Freezing-point determinations for very dilute solutions are extremely difficult, owing to the minute differences of temperature to be measured, and the results given by various observers showed great discrepancies. On the other hand, the most satisfactory experiments on the electrical ionisation of corresponding solutions had been made at higher temperatures, instead of at the freezing point, at which they should be obtained for purposes of comparison. The fact that the temperature coefficient of conductivity differs for solutions of different concentration, showed that the values of the ionisation would vary if the temperature was changed.

Mr. E. H. Griffiths therefore undertook the examination of the freezing points by the method of platinum thermometry, and the present paper contains an account of corresponding measurements of the electrical conductivities at 0° C.

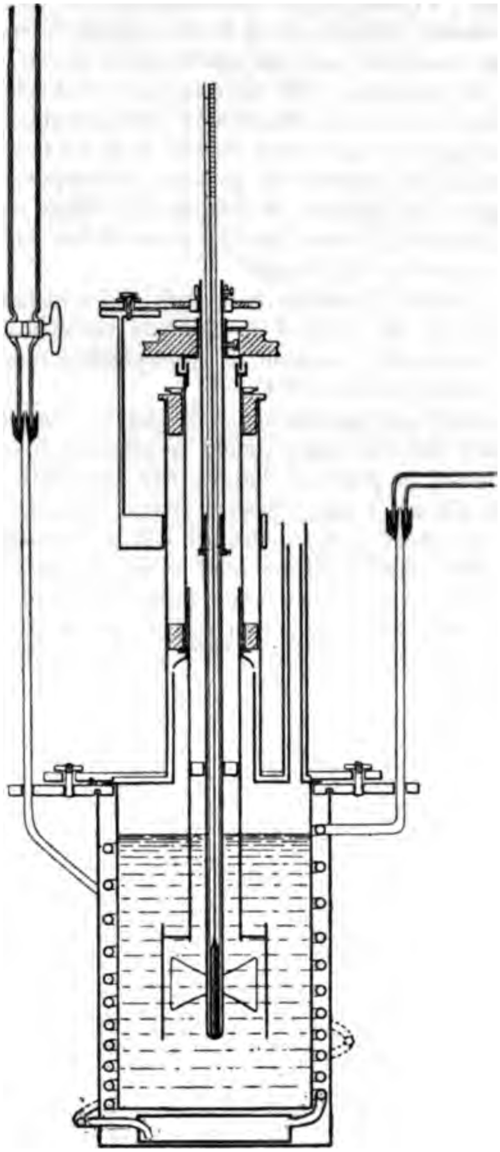
In order to avoid any possible action of glass on the solvent used, it was determined that the water should be obtained from a platinum still and collected in platinum bottles, and that both the freezing point and the electrical measurements should be made in platinum vessels. The structure of the resistance cell is represented in fig. 1. The walls of the vessel itself are used as one electrode, and an insulated platinum cage, suspended inside, forms the other. Within the cage is a platinum screw, mounted on a shaft, which can be turned by means of a hand wheel and cord. This screw is used to insure temperature equality throughout the liquid, and to mix the solutions when made. The shaft of the screw is a hollow tube, closed at the bottom, which contains a thermometer.

Instead of beginning with a strong solution and gradually diluting, it was thought better to begin with a definite quantity of the pure solvent, and, when its resistance had been observed, to add weighed amounts of stock solution of known strength by means of the platinum vessel shown in fig. 2. This vessel will obviously empty itself if a flow of liquid is started by slightly increasing the air pressure at the neck.

In order to obtain a definite quantity of solvent, slightly more than the volume needed was placed in the cell, and the level of the liquid was then adjusted by sucking water through a capillary platinum tube into the glass vessel shown in fig. 3. The bottom of the capillary always comes to the same position relatively to the cell, and, if the sucking pressure is kept constant and equal to that of a water column of about a foot in height, it is found that the amount of water left in the cell is constant to within about one-tenth of a gramme. Thus three independent withdrawals left 219·60, 219·63, and 219·59 grammes. Whenever the cell was dismounted and set up again, this measurement was repeated.

The platinum vessel was surrounded by a brass case, coils of metal

Fig. 1.



tubing being placed in the narrow air space between them. Evaporated ether vapour could be drawn through these coils by an air-pump, and thus the whole vessel cooled. The apparatus was fixed in a large copper tank, which was filled with melting ice.

The electrical resistance measurements were made by the method of alternating currents, but the usual telephone indicator was replaced by a D'Arsonval galvanometer. This was done by using a revolving commutator, which, turned by a hand-wheel and cord, alternated the connections of the bridge with the battery and with the galvanometer simultaneously. The usual Wheatstone-bridge method could then be used, and measurements obtained in the same cell of resistances varying from 10 to 50,000 ohms, the accuracy throughout being at

FIG. 3.

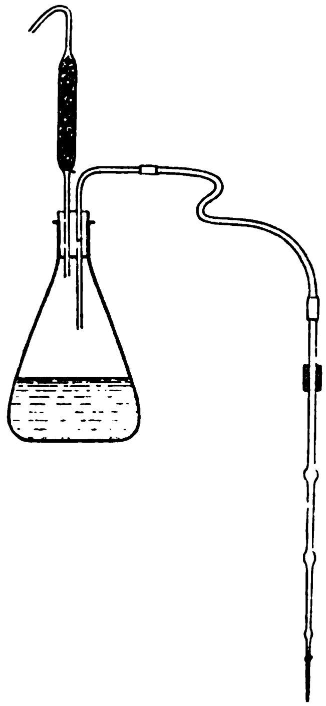
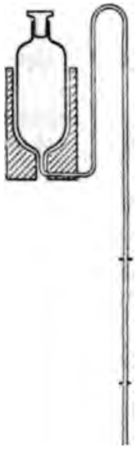


FIG. 2.



least 1 in 1000. The method eliminates several troublesome periodic disturbances, and, in this form, seems entirely satisfactory. The surface of the electrodes was platinised in the usual manner, but was afterwards heated to redness. This process gives a roughened platinum surface of large area, which is less liable to absorb matter from the solution than is the unheated platinum black.

The water used was thrice distilled, twice with alkaline permanganate and once in a platinum still with a trace of acid potassium sulphate. It had an average conductivity at 18° of about $0{\cdot}9 \times 10^{-15}$ in C.G.S. units.

Some of the stock solutions for the early part of the work were prepared by Miss D. Marshall, and most of those used in the later measurements were made up at the Cambridge University Chemical Laboratory by Mr. G. Hall, under the advice of Mr. H. J. H. Fenton. Others were prepared by the writer from recrystallised salts obtained from Kahlbaum, of Berlin.

From a knowledge of the weight of solvent used and the weight of stock solution added it was easy to calculate the concentration (m) of the resulting solution in terms of gramme-equivalents of solute per thousand grammes of *solution*. All the experiments were made on solutions so dilute that this way of defining m leads to practically the same results as though the gramme-equivalents of solute were referred to 1000 grammes of *solvent*, or to one litre of solution. The differences only become visible on the curves in the cases of two or three of the strongest solutions of some of the substances used.

The observed resistance is corrected for any slight difference in temperature from zero, and for the increased volume of liquid in the cell due to the volume of stock solution added.

The reciprocal of this corrected resistance is the conductivity in arbitrary cell units, and from this the corresponding arbitrary conductivity of the solvent is subtracted. The resultant conductivity, k, due to the added solute alone, is divided by m, and k/m, the equivalent conductivity, plotted on a diagram as ordinate, the value of $m^{\frac{1}{3}}$, a number proportional to the average nearness of the molecules, being used as abscissa. From these curves the maximum value of k/m is estimated, and taken to represent complete ionisation, the ionisation for the solutions measured being calculated as the ratio between the actual value of k/m and its maximum.

The values obtained for these ionisations were arranged as shown in the following table, which is given as an example, and are plotted as curves on the diagrams appended.

Sulphuric Acid.—Prepared at the Chemical Laboratory by adding the calculated amount of SO_3 to distilled acid. Successive crystallisation brought the melting point of the resultant H_2SO_4 to + 10·5° Cent. The crystals were dissolved in water and the concentration of the solution estimated by the barium sulphate method.

Similar measurements were made on Potassium Chloride, Barium Chloride, Copper Sulphate, Potassium Permanganate, Potassium Ferricyanide, and Potassium Bichromate.

In discussing the results, we may first notice that, in cases where it has been possible to obtain values for the ionisation at 18° from Kohlrausch's work, the ionisation curve at 0° is appreciably different from that at 18°, the ionisation falling off more rapidly with increasing concentration at the higher temperature. In the diagrams the values for 0° are given by dots inside circles, and those for 18° by crosses.

In the case of copper sulphate, measurements by the present method were made at 18° as well as at 0°, and are indicated by crosses inside circles; giving a curve which agrees with Kohlrausch's observations at moderate concentrations, but differs from them at extreme dilution.

The normal type of curve is given by potassium chloride, barium chloride, &c. The curve for sulphuric acid departs from this form, as other observers, using glass vessels and working at higher temperatures, have previously found. The drop in this curve at extreme dilution is seen also in solutions of other acids and alkalies, and it has been usual to explain it by supposing that the effective amount of acid is reduced at extreme dilution by interaction with the residual impurities of the solvent. The phenomenon seems too constant for this

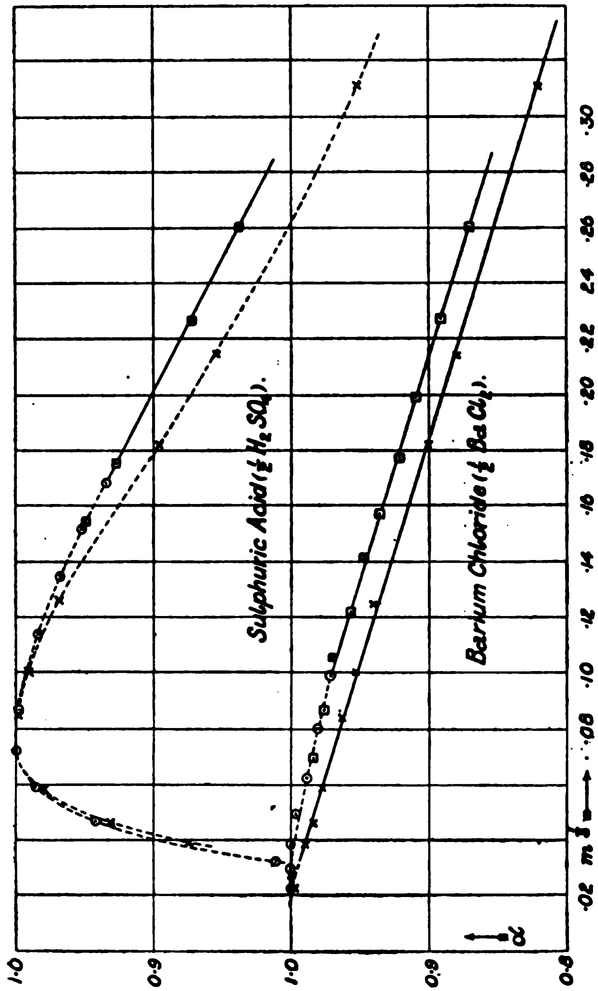
Table I.

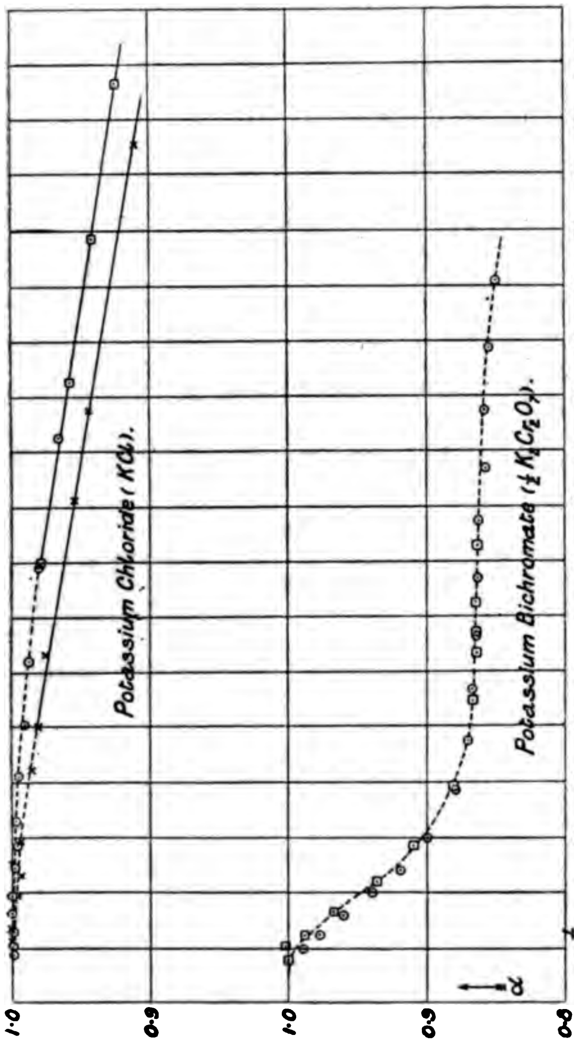
$\frac{1}{2}H_2SO_4$ = 49·04, Solvent. Weight = 219·42, R = 40420.

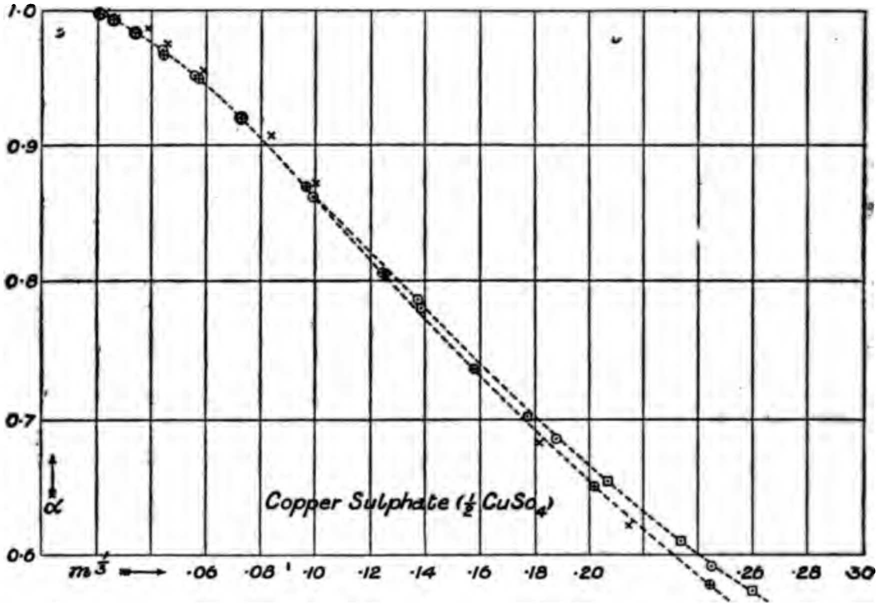
	m.	$m^{\frac{1}{3}}$.	R.	K/*m*.	*a*.
I	3·254 × 10^{-5}	0·0319	3106	9·122	0·809
II	9·628 ,,	0·0459	954·3	10·62	0·941
III	2·001 × 10^{-4}	0·0585	444·7	11·10	0·984
IV	3·558 ,,	0·0709	247·8	11·26	0·999
V	6·340 ,,	0·0859	139	11·26	0·998
VI	1·425 × 10^{-3}	0·1125	63·11	11·09	0·984
VII	2·411 ,,	0·1341	37·75	10·90	0·967
VIII	3·423 ,,	0·1507	27·25	10·73	0·951
IX	4·729 ,,	0·1678	20·05	10·53	0·934
	In Glass Cell.				
I	3·660 × 10^{-3}	0·1541	1789	0·1552	0·948
II	5·381 ,,	0·1752	1246	0·1516	0·926
III	1·158 × 10^{-2}	0·2262	615·7	0·1426	0·871
IV	1·747 ,,	0·2595	424·4	0·1347	0·837

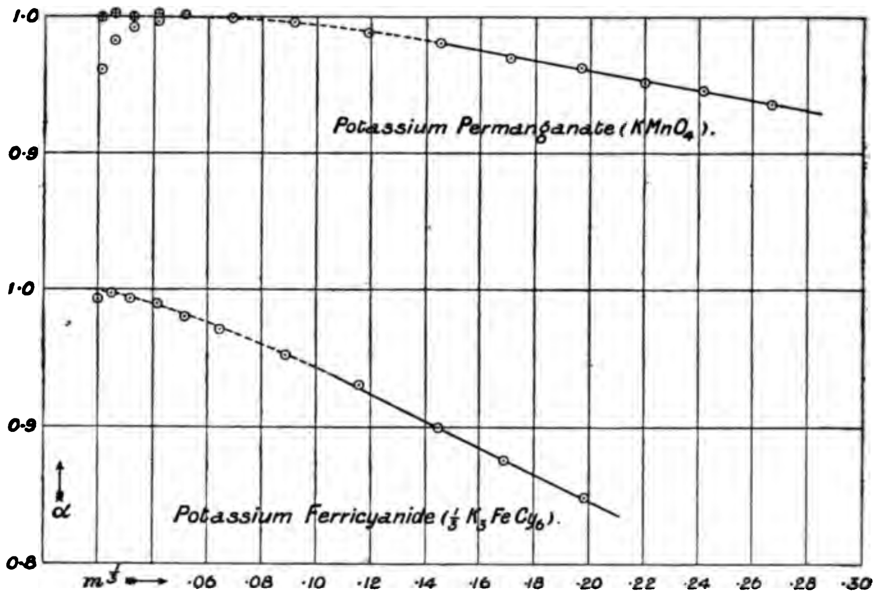
explanation to be satisfactory, and the cause of it may perhaps be connected in some way with the fact that it occurs only in solutions the solute of which gives ions either of hydrogen or hydroxyl, which are ions (1) present in the solvent, (2) possessing greater velocities than any other ions.

The drop in the curve for potassium permanganate is, on the other hand, probably due to interaction between the salt and the solvent impurities. The effect is completed by the first addition of salt, for if a correction be made in the case of the first solution for the salt thus put out of action, it is found that the curve reverts to the normal type. This is clearly shown by the diagram. Again, both in this case and in that of sulphuric acid, it was found that for solutions of great dilution the resistance showed a gradual rise for some time after the


Sulphuric Acid (½ H2 SO4).
Barium Chloride (½ Ba Cl2).
α
m
·02
·10
·12
·14
·16
·18
·20
·22
·24
·26
·28
·30
1·0
0·9
1·0
0·9
0·8


Potassium Chloride (KCl).
Potassium Bichromate (½ K₂Cr₂O₇).
1·0
0·9
1·0
0·9
0·8
α
m⅓
·00
·08
·10
·12
·14
·16
·18
·20
·22
·24
·26
·28
·30
·32
·34


Copper Sulphate ($\frac{1}{2}$ CuSo$_4$)
1·0
0·9
0·8
0·7
0·6
α
$m^{\frac{1}{3}}$
·06
·08
·10
·12
·14
·16
·18
·20
·26
·28
·30


Potassium Permanganate (KMnO$_4$).
Potassium Ferricyanide ($\frac{1}{3}$ K$_3$ Fe Cy$_6$).
1·0
0·9
1·0
0·9
0·8
α
$m^{\frac{1}{3}}$
·06
·08
·10
·12
·14
·16
·18
·20
·22
·24
·26
·28
·30

stock solution was added to the solvent, as though the action took time for its completion. While, however, in the case of permanganate this phenomenon was only observed on adding the first lot of stock solution, in the case of acid it appeared in the second solution also. This confirms the idea that the action is not completed by the first addition of acid, though the quantity of acid present must be large compared with the amount of residual impurity in the solvent.

The permanganate measurements also show that the slant of the curve is that of a salt like potassium chloride, with a monovalent acid radicle, rather than that of a salt such as copper sulphate, with a divalent acid. The chemical structure of permanganate in water solution is therefore probably represented by the formula $KMnO_4$.

The curve for potassium bichromate appears to consist of two parts, an indication, perhaps, that the ions are different at different concentrations.

In order to collect the results, smoothed values have been obtained from the curves and are appended in Tables IX, X, XI, and XII. The first three tables contain ionisation coefficients at 0°, the concentration being tabulated in different ways. Table XII shows approximate values for the equivalent conductivities at 0°. These were not necessary for the determination of ionisation, so a single value of the cell constant, obtained by comparison of the copper sulphate measurements at 18° with Kohlrausch's absolute values, was used, except for the potassium chloride solutions, which were reduced by a figure given by Kohlrausch for this salt at 0°. The errors will be small, for the amount of solvent left in the cell in each case was very nearly constant, and this is a measure of the accuracy with which the cell is readjusted after being taken to pieces. The results, however, are not supposed to be as trustworthy as those of the ionisation coefficients.

Table IX.—Ionisation Co-efficients at 0°.

m = number of gramme-equivalents of solute per thousand grammes of solution.

m.	KCl.	$\frac{1}{2}BaCl_2$.	$\frac{1}{2}H_2SO_4$.	$\frac{1}{2}CuSO_4$.	$KMnO_4$.	$\frac{1}{3}K_3FeCy_6$.	$\frac{1}{2}K_2Cr_2O_7$.
0·00001	1·000	1·000	..	0·998	1·000	0·998	0·991
0·00002	1·000	1·000	..	0·993	1·000	0·996	0·980
0·00005	1·000	0·998	0·880	0·981	1·000	0·991	0·952
0·0001	0·999	0·995	0·944	0·967	1·000	0·985	0·929
0·0002	0·998	0·990	0·984	0·947	0·999	0·977	0·902
0·0005	0·996	0·980	1·000	0·908	0·998	0·961	0·880
0·001	0·992	0·969	0·992	0·863	0·993	0·944	0·870
0·002	0·987	0·953	0·974	0·807	0·986	0·919	0·864
0·005	0·976	0·925	0·931	0·717	0·971	0·876	0·863
0·01	0·962	0·896	0·883	0·638	0·955	0·834	0·858
0·015	0·952	0·876	0·851	0·591	0·944	..	0·853
0·02	0·944	0·860	0·825	0·557	0·934	..	0·847
0·03	0·932	0·833	0·784	0·509			

Table X.—Ionisation Co-efficients at 0°.

m = number of gramme-equivalents of solute per thousand grammes of solution.

m.	KCl.	$\frac{1}{2}BaCl_2$.	$\frac{1}{2}H_2SO_4$.	$\frac{1}{2}CuSO_4$.	$KMnO_4$.	$\frac{1}{3}K_3FeCy_6$.	$\frac{1}{2}K_2Cr_2O_7$.
$\frac{1}{131072}$	1·000	1·000	..	0·999	1·000	0·999	0·994
$\frac{1}{65536}$	1·000	1·000	..	0·996	1·000	0·997	0·985
$\frac{1}{32768}$	1·000	0·999	0·795	0·988	1·000	0·994	0·969
$\frac{1}{16384}$	1·000	0·997	0·901	0·978	1·000	0·990	0·947
$\frac{1}{8192}$	0·999	0·994	0·958	0·963	1·000	0·983	0·921
$\frac{1}{4096}$	0·998	0·988	0·991	0·940	0·998	0·974	0·895
$\frac{1}{2048}$	0·995	0·980	1·000	0·909	0·996	0·962	0·880
$\frac{1}{1024}$	0·992	0·969	0·992	0·865	0·993	0·944	0·870
$\frac{1}{512}$	0·988	0·954	0·974	0·809	0·987	0·920	0·865
$\frac{1}{256}$	0·979	0·934	0·944	0·742	0·977	0·889	0·863
$\frac{1}{128}$	0·968	0·908	0·901	0·666	0·962	0·850	0·861
$\frac{1}{64}$	0·951	0·875	0·847	0·587	0·942	..	0·852
$\frac{1}{32}$	0·930	0·831	0·779	0·505			

Table XI.—Ionisation Co-efficients at 0°.

n = number of gramme-molecules of solvent per gramme-molecule of solute.

n.	KCl.	$\frac{1}{2}BaCl_2$.	$\frac{1}{2}H_2SO_4$.	$\frac{1}{2}CuSO_4$.	$KMnO_4$.	$\frac{1}{3}K_3FeCy_6$.	$\frac{1}{2}K_2Cr_2O_7$.
20000000	..	..	..	..	..	0·998	..
10000000	..	1·000	..	0·997	..	0·997	0·990
5000000	1·000	1·000	..	0·992	1·000	0·994	0·976
2000000	1·000	0·998	0·894	0·979	1·000	0·987	0·949
1000000	1·000	0·994	0·950	0·965	1·000	0·979	0·925
500000	0·999	0·989	0·989	0·944	1·000	0·969	0·901
200000	0·997	0·979	1·000	0·902	0·999	0·949	0·878
100000	0·994	0·966	0·989	0·856	0·997	0·926	0·870
50000	0·992	0·950	0·970	0·797	0·993	0·897	0·866
20000	0·984	0·922	0·924	0·705	0·982	0·846	0·862
10000	0·974	0·893	0·875	0·626	0·969	..	0·858
5000	0·960	0·857	0·814	0·545	0·952	..	0·846
2000	0·935	..	..	..	0·923		

Table XII.—Approximate Equivalent Conductivities at 0°, in C.G.S. units × 10^{13}.

m = number of gramme-equivalents of solute per thousand grammes of solution.

m.	KCl.	$\frac{1}{2}BaCl_2$.	$\frac{1}{2}H_2SO_4$.	$\frac{1}{2}CuSO_4$.	$KMnO_4$.	$\frac{1}{3}K_3FeCy_6$.	$\frac{1}{2}K_2Cr_2O_7$.
0·00001	807	765	..	704	756	964	833
0·00002	807	764	..	701	756	962	823
0·00005	807	763	2309	692	756	957	800
0·0001	806	761	2477	682	756	951	780
0·0002	805	757	2582	668	755	943	758
0·0005	804	750	2624	641	755	928	739
0·001	800	741	2603	609	751	911	731
0·002	796	729	2556	569	746	887	726
0·005	787	708	2443	506	735	846	725
0·01	776	685	2317	450	722	805	721
0·015	768	671	2233	417	714	..	716
0·02	761	658	2165	398	706	..	711
0·03	752	638	2057	359			

"On the Relation of Artificial Colour-blindness to Successive Contrast."

By GEORGE J. BURCH, M.A. Oxon., Reading College, Reading. Communicated by Professor GOTCH, F.R.S.
Received January 30—Read February 8, 1900.

I have elsewhere pointed out that my observations on artificial colour-blindness seem unfavourable to the theory of Hering, and favourable to that of Young. The experiments on successive contrast described in the following pages tend also to confirm in a remarkable manner opinions held before the time of Young, and which must be considered as incorporated in his theory. The method I have pursued throughout this investigation consists essentially in the use of the spectroscope to analyse sensations of contrast, and I have accordingly been able to make certain experiments which would have been beyond the resources of those earlier writers; but the following account of their opinions, expressed as far as possible in the words of the authors themselves, is intended to show in some detail how closely their views agree with my own results.

During the 18th century the phenomena of after-images and successive contrast attracted a good deal of attention, and although in most cases the physical conditions of the experiments were too complex to afford much information as to their true nature, there were some remarkable exceptions, to which I desire to direct attention.

The opinions enunciated during the 18th century may be divided broadly into two groups. The one school held that after the stimulus of a strong light of any given colour, a species of reaction sets in, by which a sensation of the complementary colour is produced. This view may be regarded as belonging to the same category as the theory of Hering. It may be doubted whether it was definitely adopted by Jurin,* who says only that this "contrary sensation is apt to arise in us sometimes of itself, and sometimes from such causes as at another time would not produce the sensation at all, or at least not to the same degree," and preserves a like caution throughout his description of the phenomena. Other experimenters, however, advocated this theory, and it was strongly upheld in 1801 by Venturi,† who maintained that the changing tints of after-images excited by the pure colours of the spectrum proved the existence of a multiple function for each nerve-fibre, as opposed to the theory of one nerve, one function, taught by Bonnet.‡

* "Essay on Distinct and Indistinct Vision." In Smith's 'Opticks,' 1738.

† "Dei Colori Immaginarii." 'Opusc. scelti sulle Scienze,' da Carlo Amoretti. Soave, vol 21, p. 274.

‡ "Essai Analytique sur l'Âme," and "Essai de Psychologie." Works. 1785.

The other school, from which the theory of Young may be considered to have developed, seems to have been founded by Scherffer, who, in 1761, published a long series of experiments on contrast based on Buffon's work, but criticising his conclusions. Scherffer's standpoint is briefly expressed in the following passage:—

"Perhaps the Creator has so constructed the entire organ of vision that each kind of ray can only act upon such of the parts of which the eye is composed as are particularly appropriated to it. But I presuppose that the whole action of light consists in attraction and repulsion. . . . . It may be that a continuous action of, for instance, red light, may so change the order and arrangement of the parts of the back of the eye . . . . that those rays may be no longer strong enough to communicate to these parts the necessary vibratory movement, until a little rest shall have restored them to their condition . . . . and during this time the other rays of different kinds will not cease to act . . . ."

He points out that if this explanation of "accidental colours" is the true one, it must follow that the after-image of a coloured object viewed upon a ground of the same colour must be black, just as a white spot upon a dark ground gives a black after-image upon white paper.*

Two of his experiments may be specially noted:—

In order to determine the complementaries of the primary colours by experiment, he projected the solar spectrum on a white surface, and observed the colours of the after-image produced by it.† He then compared these with the corresponding colours, as calculated by Newton's method. He gives the following list of colours observed:—

Red	Blue, verging on green.
Orange	Blue, almost indigo.
Yellow	A more violet-blue.
Green	Purple.
Blue	Red.
Indigo	Orange, but rather pale.
Violet	A very yellow green.

He also made drawings of flowers, and painted them with colours complementary to those they naturally possessed. These, when steadily looked at in a bright light, gave after-images in their true colours. He even went so far as to copy a picture, painting it with a green face shaded with yellow, white hair and eyebrows, black eye-balls with white pupils, and green lips, so that the accidental image of it had the colours of the original.‡

* Compare this paper, Section II (2), p. 208.

† Compare this paper, Section II (1), p. 208.

‡ Compare this paper, Section III, p. 213.

He does not, however, deal with the positive after-image and after-effects. This was done by Robert Waring Darwin,* in his paper on the "Ocular Spectra of Light and Colours," in which he treats of the "direct and reverse spectra" of brightly illuminated pieces of silk of various colours. He describes very clearly the series of changes of these after-images from negative to positive and back again, observable under certain conditions, and points out that in order to see the direct spectrum (positive after-image) all extraneous light must be excluded, whereas "it is difficult to gain the reverse spectrum (negative after-image) where there is no lateral light to contribute to its formation." "The reverse spectrum is instantaneously converted into the direct spectrum by excluding lateral light, and the direct into the reverse by admitting it. . . . ." "The green spectrum which is perceived on removing the eye from a piece of red silk to a sheet of white paper, may either be called the reverse spectrum of the red silk, or the direct spectrum of all the rays from the white paper except the red, for in truth it is both." Thus the "direct spectrum" is the sensation of each colour persisting after the cause that produced it has ceased to act, and the "reverse spectrum" is the effect of compound colours upon the retina, which still remains liable to be excited "by any other colours except the colour with which it has been fatigued." He proves this by showing that the colour of the "reverse spectrum" depends upon that of the "lateral light," *i.e.*, the light which reaches the eye after the retina has been fatigued.† He compares these phenomena with those of taste, touch, and hearing, and shows that each of these senses undergoes a partial temporary paralysis after being strongly excited. Although, therefore, the unaided evidence of the senses might have suggested that each pair of complementary colour-sensations, such as red and green, or blue and yellow, were conjugate functions of some nerve structure, it is plain that he desired to emphasise the fact that they must be regarded as due to separate nerve structures. One nerve, one sensation. The sensation might be weak or strong, according to the physiological condition of the organ at the time, but its character could not be changed.

Darwin, following Newton, refers to seven colours as primaries. In 1792 Wünsch,‡ whose method consisted in the superposition of spectra projected upon a screen, stated that a mixture of three colours, namely, red, green, and violet-blue, could be made to match any given tint.

For the basis of Young's theory there existed, therefore, experimental evidence of the small number of the primary colour-sensations, and of their being functions each of some nerve structure specially

* 'Phil. Trans.,' vol. 76 (1786), p. 313.

† Compare this paper, Section II (1), p. 208, and Section III, pp. 212, 216.

‡ 'Ueber die Farben des Lichtes.' Leipzig, 1792.

appropriated to it. The phenomena of negative and positive after-images were known—it was known that certain colours are altered in hue to an eye previously exposed to coloured light, and that experiments on this subject should be made with the spectrum rather than with pigments.

The following papers also are of special interest in connection with the present communication.

Brewster,* by looking at the spectrum through a coloured medium, was able to trace the green as far as C. His mode of explaining the phenomenon led to a controversy, in which the true merit of the observation was lost sight of. A momentary colour-blindness is, in reality, as was shown by Hunt, produced by the contrast of adjacent parts of the spectrum differing greatly in brightness.

Piazzi Smyth,† working with a very long spectrum, observed the boundaries of the colours to change when any alteration was made in the intensity of the illumination.

The earliest account of a systematic investigation of the effect of retinal fatigue on the colours of the spectrum is in a paper of John Aitken.‡ Similar observations were made by Edmund Hunt,§ who also describes the appearance of the spectrum when observed through certain coloured media, after the manner of Brewster. Coloured figures of the results are given. Both these authors used light much less intense than that employed by me, and did not obtain the full effect. I was not aware of their work until after my own paper had been read, and therefore take this opportunity of calling attention to it. To these may be added a paper by Hess‖ on the Alterations of the spectral colours by retinal fatigue.

II.—*Experimental Investigation of the Phenomena of Successive Contrast.*

Successive contrast is an effect of two stimuli—a primary stimulus by which the retina is fatigued, and a secondary stimulus, the effect of which is modified in consequence of the first. The colour-sensations excited may be reduced to four at the most. To arrive at the fundamental laws of contrast, we may vary the conditions in the following manner:—

Let the first stimulus excite a single colour-sensation, taking each in turn, or separated from the rest as in the spectrum. Four cases arise:

(1.) The second stimulus may excite all the sensations, *i.e.*, it may consist of white light.

* 'Edin. Trans.,' vol. 12 (1834), p. 132.

† 'Roy. Soc. Edin. Trans.,' vol 28 (1879), p. 792.

‡ "Colour and Colour-Sensation," 'Roy. Scot. Soc. of Arts Proc.,' 1871-72.

§ Hunt, 'Colour Vision,' Glasgow, 1892.

‖ Graefe's 'Archiv für Ophthalmologie,' 36, abth. 1, pp. 1—32.

This gives the complementary colour, *i.e.*, the direct spectrum, as Darwin calls it, of all the colours save that excited by the first stimulus.

(2.) The second stimulus may excite the same sensation as the first, but less strongly.

This gives a black after-image.

(3.) The second stimulus may excite two or more colour-sensations, including that of the first stimulus.

The colour of the resulting image is that of the second stimulus *minus* the first.

(4.) The second stimulus may excite one or more sensations, none of which were included in the first stimulus.

The colour of the resulting image is that of the second stimulus *plus* an admixture, usually small, of the first.

This scheme is covered by the experiments described in the following pages. They are so arranged as to require but little special apparatus, and to employ spectral colours by direct observation. Some little care must be taken to adjust the relative intensity of the two stimuli correctly, and to effect the change from one to the other as suddenly as possible.

1. *After fatiguing the retina by the spectrum, to observe a uniform white light.*

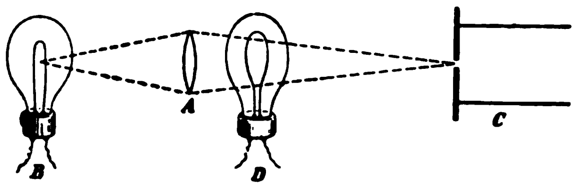
This is most easily done by means of a low-power spectroscope with a reflected-scale tube. Unscrew the cap containing the scale, and place a mirror so as to reflect white light from the sky into the tube. Cover the open end of the scale tube by a black card held in the right hand, and have a similar card in the left hand in readiness to cover the slit of the spectroscope. Let the slit be fairly wide so as to give a rather bright spectrum. Look steadily at the spectrum for half a minute, keeping the eye fixed on the intersection of the cross wires, and then suddenly cover the slit and uncover the scale tube. A complementary spectrum will be seen, brilliantly defined, for a fraction of a second. To myself, by daylight the spectral red is replaced not by green, but by blue, and the complementary of green is a pinkish purple, but by lamp-light the complementary of red is green, and that of green is red. The advantage of this mode of experimenting is that it utilises existing apparatus.

2. *After fatiguing the retina by the spectrum to observe a less intense spectrum.*

The phenomenon of successive contrast is shown by the preceding method in its least simple form. To analyse it, the effect of retinal fatigue by each spectral colour on the perception of that same colour must be determined. This may be done by focussing with a lens A (fig. 1) a glow lamp B on the slit C of an ordinary spectroscope, and at the same time illuminating it by a second glow lamp D placed between

the lens and the slit. The effect produced is that of a broad, continuous spectrum, with a narrow but much brighter spectrum in the middle of it. After a few seconds a black card is suddenly brought behind the lens, so as to screen off the light of the focussed lamp B. A dark band like a shadow instantly appears in place of the narrow bright spectrum—that is to say, *the effect upon the retina of light of any wave-length is to blind the eye temporarily for light of that same wave-length.* This may be illustrated in another way. Place near the slit of the spectroscope a

FIG. 1.



Bunsen burner, and behind it, a few inches farther off, a lamp, and hold between the lamp and the Bunsen flame a black card. Burn some calcium or strontium chloride, or common salt, or anything that gives a good bright-line spectrum, in the Bunsen flame, keeping the eye fixed on one of the lines. On snatching away the card and the Bunsen flame a dark-line spectrum will be seen momentarily against the continuous spectrum of the lamp, so sharply defined that it is difficult to realise that it is merely an illusion.

These results are of cardinal importance. They mean that the green or blue subjective impression produced by a white surface when the eye has been fatigued for red does not indicate that red excites an after-sensation of green or blue, or renders the eye more sensitive to green or blue, but that the eye has become less sensitive to red. And similarly with the other colours. This point is clearly brought out by Darwin. An "accidental" colour has therefore this in common with an absorption spectrum—that it involves a diminution of the intensity of a certain portion or portions of the spectrum.

The line of proof is completed by the third disposition of the variables.

3. *After fatiguing the retina by any one colour, to observe the entire spectrum.*

This is in effect a mere variant of the method described in my paper,* and depends on the production of a very transient colour-blindness. It is necessary to make special arrangements for suddenly substituting a complete spectrum for a field of view illuminated by monochromatic light. Among the methods I have tried, the following may be mentioned :—

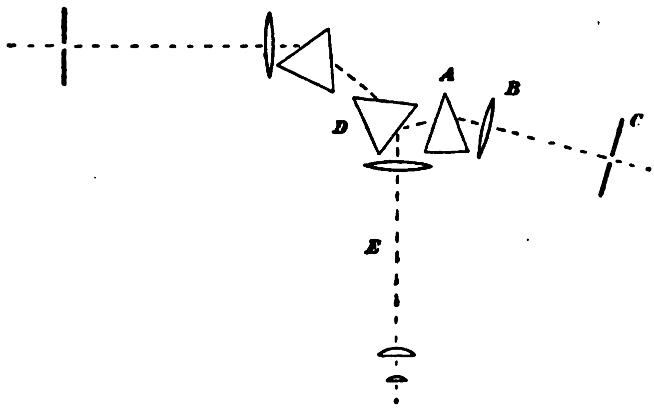
* 'Phil. Trans.,' B, vol. 191 (1899), p. 4.

(1.) Place a coloured screen over the end of the scale tube of an ordinary spectroscope, the scale being removed, and cover the slit with a card. After looking at the coloured light for some seconds, cover the scale tube, and simultaneously uncover the slit. This experiment is easily tried, but is open to the objection that the first stimulus is not perfectly monochromatic.

(2.) Illuminate the scale tube with monochromatic light from a prism, and proceed as before This plan obviates the difficulty referred to, but does not afford sufficient light to produce the ful effect.

(3.) A single prism A, fig. 2, with collimator B, and slit C, is fixed

Fig. 2.



near the back surface of the last prism D of the large spectroscope in such a position that the rays from it are reflected into the telescope E of the large spectroscope. This second spectrum is of course much fainter than the one observed directly through the instrument, but that is an advantage rather than otherwise. It is only necessary to arrange two black cards with slits in them in such a way that when light passes through the first spectroscope the second is obscured, and on touching a spring the conditions are reversed.

In order to make the effects more marked, a short slit should be used for the large spectroscope, so that it may give a band of monochromatic light across the middle of the field, fairly bright but rather narrow. The eye should be fixed on the centre of this band. After a few moments, by the action of the spring referred to, a black card is suddenly brought over the slit of the spectroscope, shutting off the light, while at the same moment the screen is removed from the other spectroscope, and the complete spectrum appears, filling the entire field of view. For an instant, a dark shadow is seen, not extending

across the entire spectrum, but only that part of it corresponding to the colour-sensation excited by the monochromatic light. The effect is very striking after red light. An intensely black band cuts through the spectrum from the ultra-red, as far as C, where it begins to fade away into a pure green.

After violet light, a similar black band cuts through the spectrum from the ultra-violet, and if care has been taken not to implicate the blue in the fatigue, the black band fades away into blue.

After green light, most frequently the red and blue are seen to stretch across and meet in the middle of the *b* lines; but sometimes, if the exposure is exactly right, a well-marked darkening of that part of the spectrum is seen.

Blue light is the most difficult to manage, unless a wide dispersion is used, the blue being otherwise not sufficiently separated from the green and the violet. After getting the adjustments right, it is better either to wait ten minutes, or use the other eye. With these precautions, it is easy to see the green and violet meet in the place of the blue, and to note that the after-image of the blue casts no shadow on the violet near H. Sometimes a momentary shadow may be seen in the blue. This experiment is of interest as affording additional evidence of the existence of a separate sensation for blue.

It should be noted that the two spectra must have the corresponding colours on the same side. If the prism of the second spectroscope is reversed so as to bring the red of one spectrum towards the violet of the other, a black shade, very well defined, can be produced in *any part of the second spectrum*. For if the two spectra are so arranged that any given portion of the one corresponds with the same wave-length on the other, then no part of the one spectrum on either side of that one part will be of equal wave-length with the portion of the other spectrum which coincides with it. Accordingly, a negative after-image will be produced only of the short space within which the wave-lengths are approximately the same. But it is clear that such an experiment is more curious than useful.

III. *Contrast Phenomena by Intermittent Stimulation.*

In 1868 Sigmund Exner* made a series of experiments on the following plan. After an interval of darkness, he presented to the eye, for a fraction of a second, the image of a small white disc. This was succeeded by a disc of considerably larger diameter, which in turn was followed by darkness. The illumination of either disc, and the period during which it was visible, could be independently varied. Thus the portion of the retina on which fell the image of the small disc received

* Exner, 'Sitzungsberichte d. Wiener Akad.' Abth. 2, vol. 58 (1868), pp. 601—632.

not merely the light of the larger disc, but in addition the light of the smaller disc. Yet in spite of this, with certain relations of intensity and duration between the two images, the total sensation evoked by the larger quantity of light was less, so that the small disc appeared as a black spot on the larger disc. I was curious to ascertain to what extent this principle could be carried. By the following experiment it may be demonstrated in a striking manner. A cardboard disc, A, figs. 3, 4, 200 mm. diameter, of which 180° is black and

FIG. 3.

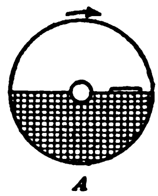
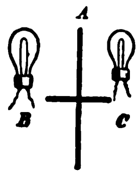


FIG. 4.



the rest white, has a slit about half a millimetre wide cut at the junction of black with white. It is made to revolve so that black precedes and white follows the slit. An incandescent lamp B is placed behind the disc, and another C in front of it, so as to throw a strong light upon its surface. While the disc is moving slowly the incandescent filament of the lamp B, seen through the slit as it passes across, looks bright against the white card, but when a certain speed is reached it appears as a black thread against a brighter background.

In spite of the very short duration of the intense light of the filament, the fatigue induced by it is out of all proportion to the sensation it excites, and in consequence the less fatiguing illumination of the white card produces a greater effect on the senses than the sum of the sensations due to the filament and the subsequent light. With a single flash the filament looks, to the rested eye, black all over, but with a succession of flashes there is generally an appearance as though the luminous filament were partly covered by an opaque black thread, but could be seen in places behind it. This I think is due to the shifting of the images on the retina, which after the first flash is no longer in a uniform condition. The retina is, as it were, scarred with after-images, and the ratio of illumination to length of flash which suits one part is incorrect for another.

When a red glass is placed over the lamp the reversed image of the filament is green if the card is not quite white, or if the light falling on it is yellowish, but blue-green or blue if it is illuminated by sunlight or the arc lamp. The use of coloured glasses, however, so far diminishes the light that in most cases a disc with a different flash ratio has to be employed, and the lamp C placed at a greater distance.

I next attempted to get the reversed image of the sun. This was attended with difficulty, owing to the lack of a disc with a slit sufficiently fine to reduce the sensation evoked by the direct light of the sun within the limits required. I succeeded at last by increasing the intensity of the illumination of the white card. This was effected by holding in front of it a large lens by which the sun's rays could be concentrated, the degree of concentration being regulated by adjusting the distance of the lens from the card. In this way I was able to see the sun's disc black upon a white ground. The experiment tended to confirm the explanation already given of the appearance of the incandescent filament under similar conditions. If the visual axis was not fixed, three or four black discs would appear, and on looking directly at one of the more central ones, the sun's disc seemed to be partly visible behind it. A very curious effect was produced by "sweeping" with the eye along a faint circle marked on the revolving card. A whole series of black discs started into view one after the other without a glimpse of the luminous disc that produced them.

The principle underlying Sigmund Exner's method is illustrated in an even more striking manner by the remarkable experiment of Shelford Bidwell. In this a coloured object is placed behind a disc half black and half white, with a sector 30° wide cut out of the white portion. As the disc revolves, the eye is kept in darkness for a space, then sees the coloured object for a short time, and immediately afterwards a white surface for a considerably longer time. The retinal fatigue induced by the colours of the object causes a negative after-effect so strong that the object is seen in its complementary colours.

From the point of view of my own investigations it was necessary to repeat these experiments with the pure colours of the spectrum. There are several positions in which a disc, such as Shelford Bidwell employs, can be used in conjunction with a spectroscope. It may be placed between the prism and the telescope, the latter being set back an inch or two to make room for it, or it may work in a gap cut in the body of the telescope, being illuminated by front light through a side tube. But either arrangement involves some alteration of the spectroscope. The following method is free from this objection and has a certain interest of its own :—

The disc is placed in front of the eye-piece of the spectroscope, and the spectrum viewed through a second telescope fixed in the optic axis an inch or two from the eye-piece. But the second telescope magnifies the spectrum and consequently renders it less bright. The definition is, however, much better than would be expected, and is so little affected by slight displacement of the second telescope from the optic axis that it occurred to me to try the arrangement shown in figs. 5, 6.

A telescope A, magnifying ten times, is placed with its eye-piéce close to the eye-piece E of the spectroscope. The disc B revolves

between the objective of A and the objective of a second telescop C, magnifying five times. The first telescope A being inverted diminishes the image to one-tenth of its size, and the second tele scope only magnifies it five times, so that it appears to the ey half the size it would without the telescopes, and correspondingl brighter. Although the two telescopes were merely supported b

Fig. 5.

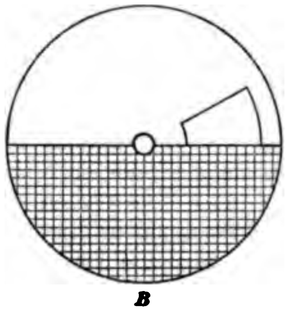
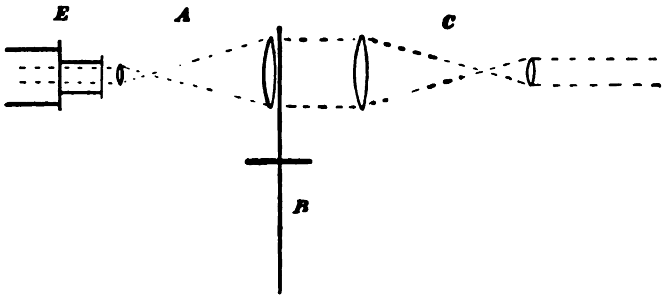


Fig. 6.



retort stands and roughly adjusted, the definition was quite goc enough and the light strong enough to show the complementa spectrum extremely well.

But the white light used was merely that reflected from card, ar was in consequence weak in the extreme violet rays. By the followir arrangement white light reflected from a mirror may be employed:- The disc A, figs. 7, 8, which is 25 cms. in diameter, has two sectors 30° aperture, and reaching within 2 cms. of the centre, cut away opposite ends of a diameter. The disc B, 15 cms. in diameter, has tv narrow slits of about 1° or 2° aperture and 180° apart. Both discs a blăcked and mounted upon the same shaft, which is furnished with nut and broad washer, so that they can be clamped together. Tl

shaft is so fixed that the slits of the smaller disc may revolve close in front of the slit C of the spectroscope, an ordinary single-prism instrument, furnished with a reflected-scale tube, the scale being removed, leaving the tube open. A mirror placed at D in front of the sectors of the larger disc reflects light from the sky on to a second mirror E, by which it is reflected into the scale tube, causing the field of view to be filled with a soft white light.

FIG. 7.

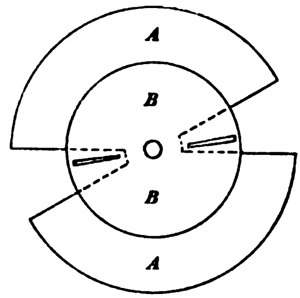
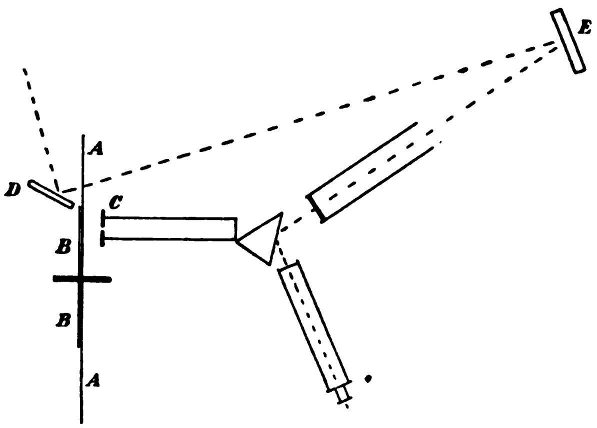


FIG. 8.



The mirrors and discs must be adjusted until on rotating the shaft slowly the flashes occur in the following order:—First a sharp flash through the small disc, giving a momentary view of the spectrum. As soon as possible after this is over, but not before, there is a rather long soft flash of pure white light, followed by a much longer period of perfect darkness. The reason for having two slits and two sectors on the discs is simply that they may be well balanced on the shaft, and therefore rotate more steadily. There should be no overlapping

of the spectral flash by the white flash, a short interval of darkness between them being preferable to the smallest overlap. For this reason the shaft is fitted with a screw nut, which being slackened, the angular position of the slits with respect to the sectors can be accurately adjusted. On rotating the discs steadily, but not too quickly, a spectrum of complementary colours is seen with the greatest distinctness. By placing a narrow strip of black card across the mouth of the scale tube, a portion of the white flash may be stopped out, allowing the normal spectrum to be seen in that part of the field. It is necessary, however, to shade the corresponding part of the slit somewhat, so that the normal spectrum may not overpower the complementary spectrum. The colours as I see them are as follows:—Red is replaced by Prussian blue, green by purple (a red shade of Hoffmann's violet), blue by orange, and violet by yellow. To show the complementary of violet it is necessary to use sunlight, or, better still, the arc light. I have never been able to see it properly by any of the methods involving the use of white card or paper surfaces as reflectors.

The experiments of Section 3, for which a wide dispersion was required, were made with a large direct-vision spectroscope belonging to the Marlborough Collection, for the use of which I am indebted to the Aldrichian Demonstrator of Chemistry, Mr. W. W. Fisher. I have also to thank Professor Gotch for the use of the electric light in the physiological laboratory. The remainder of the work was done at Reading College, and the expenses have been defrayed by a portion of the sum of £10 allotted to me by the Royal Society out of the Government Grant.

"On the Production of Artificial Colour-blindness by Moonlight."

By GEORGE J. BURCH, M.A. Oxon., Reading College, Reading. Communicated by Professor GOTCH, F.R.S. Received January 30,—Read February 8, 1900.

Since the publication of my paper on "Artificial Colour-blindness"* I have found a very general and not unnatural tendency to regard the results described therein as phenomena of a pathological condition induced by the severe strain to which the structures of the eye had been subjected. In my paper I indicated, perhaps too briefly, that this could not be the case, since "the same general phenomena are observable alike with strong sunlight and with the faintest light the eye is capable of perceiving."

The purpose therefore of the present communication is to describe some of the experiments on which that statement was based.

* 'Phil. Trans.,' B, vol. 191 (1899), p. 1.

When green-blindness is induced by exposure of the eye to intense green light, not only is the observer unable to perceive the colour of green objects, but the sensation of green is no longer excited by the intense green light that caused the blindness. And the same may be said of blue-blindness. On the other hand, with artificial red-blindness the exciting light still looks reddish, though greatly dulled and much paler in hue, but all objects less brightly illuminated fail to excite the red sensation. Probably in the case of green-blindness the green sensation is not entirely destroyed, but reduced so much that the red and blue sensations, which are also excited by that same part of the spectrum, completely overpower it. In producing red-blindness, as there are no colours to the left of red, I have generally used a part of the spectrum which excites only the red sensation, and which therefore must continue to appear red if visible at all. But I have not thought it desirable to push the fatigue of the retina far enough to destroy the sensation of light.

For the mere demonstration of the phenomena of colour-blindness, light of quite moderate intensity is amply sufficient if the precaution is taken of shielding the eye from all other light during the experiment, and of giving it time to recover from the effects of previous illumination. The colour-blindness so produced is, however, not absolute, but merely relative, the sensation which has been fatigued, whether red, green, blue, or violet, being still excited by a stronger stimulus.

The following is perhaps the most striking and suggestive way of making the experiment :—

1. I exposed my left eye to direct moonlight in the focus of a lens behind a screen of ruby glass combined with a gelatine film stained with magenta. After three minutes I looked through a spectroscope directed to the moon. The red had entirely disappeared, and only the green, blue, and violet were visible. With the right eye I could see the red as well as the other colours.

2. I exposed my right eye in the same manner to moonlight, using a screen of green glass instead of the red. On looking through the spectroscope I found the green sensation had entirely vanished, the red meeting the blue in the same part of the spectrum, viz., between E and *b*, as in my experiments with sunlight.* The violet was easily distinguished from the blue in this case also. The left eye was still partially red-blind, and the contrast between the spectrum as seen by it and by the right eye was very marked.

I was unable to use spectral colours for fatiguing the eye because the full moon is not visible at this season of the year from the laboratory in which the large spectroscope is mounted, and the intensity of the light was too much reduced by reflection from the two mirrors of the

* 'Phil. Trans.,' B, vol. 191, Plate I, figs. 4 and 5.

heliostat. I did not therefore make any observation with blue o: violet light.

As may be imagined, it is necessary to use a larger lens with moon light than with sunlight. In practice I have found an ordinary readin lens, of 4 inches diameter, sufficient. To obtain the full intensity of illu mination, the focal length should be such that the moon's image ma be not smaller than the pupil of the eye.

There are two points of interest in connection with this experiment The first is that the illumination of surrounding objects is on the sam scale, as regards contrast of light and shade, in moonlight as in sun light—that is to say, in each case the source of light is an object sub tending an angle of about 30′ at a distance which is practicall infinite. Whatever difference may seem to exist must be of physic logical or psychical origin. The deeper shadows in moonlight probabl afford too little stimulus to fully excite the sensation of vision even i an eye accustomed to darkness; but it must not be forgotten that w accentuate this difference by a habit of looking at the moon itself an at the bright sky near it, thus blinding ourselves to the faintly illum nated details of the shadows. If we were to do the same with sunligh the shadows would seem equally lacking in detail. In a room art ficially lighted there is seldom so much contrast between lights an shadows. Light-coloured objects are usually to be found in clos proximity to the lamps, even where white shades or globes are not use to diffuse the light. It is less easy to demonstrate the phenomena c temporary colour-blindness under these circumstances, owing to th greater relative intensity of the dazzle-tints* resulting from the actio of the diffused light before the experiment began. Until these are gon the retinal fatigue is not confined to one colour. If in experimentin with moonlight the observer accidentally looks at the moon's dis before his eye is protected by the coloured screen, a well-defined afte image is produced, and the subsequent phenomena of colour-blindnes are only locally modified, whereas if an after-effect even of les intensity, due to diffused light, is present, the colour-blindness may b to a great extent masked.

The other point of interest in connection with this experiment that colour-blindness has been produced by light no stronger than th reflected by ordinary pigments in sunshine. That this is so is evider if we look at the moon's disc in the daytime through the same re glass and lens and compare it with a piece of coloured paper. It ca therefore be hardly maintained that the condition of temporary colou blindness should be regarded as a pathological result of excessiv stimulation of the colour sensations. Merely to look for a few secon

* It will be convenient, in describing my own experiments, to retain this wor which I have used to signify the "elementary component sensations of t positive after-effect." 'Phil. Trans.,' B, vol. 191 (1899), p. 6.

at a scarlet poppy in a cornfield causes a measurable degree of red-blindness for the next two or three minutes. In applying the spectroscopic method of measuring the colour sensations described in my previous paper* it is necessary to guard against this source of error. Last summer I had a case of a man who seemed at first to be very nearly green-blind. His green sensation did not reach more than half way between *b* and F, even after fatiguing for thirty seconds with blue light, and it was correspondingly shortened on the red side. After conversing with him for some time in the subdued light of the laboratory I repeated the measurements, and found that his green sensation then extended considerably beyond F. It appeared that he had been strolling about the Parks on the grass in the bright sunshine. I have myself frequently experienced a temporary green-blindness from a similar cause. The effect seems to be intensified by looking at a white surface, as, for instance, in reading a book while sitting on the grass. After a time the green leaves seem to lose their colour and become greyish. This effect may be often noticed during a long walk through the fields. If during this condition the eyes are directed to a small red spot on a black surface, as, for instance, a single geranium petal on the black cover of a book, and the observer walks with it quickly into a dark shed or barn, the colour of the geranium petal will seem to change from red to orange and then to yellow, and finally almost whitish, owing to the subjective admixture with the red of the green dazzle-tint. On coming out into the light again the red colour will reappear. These changes are similar to those observed in the red end of the spectrum during green-blindness on opening and closing the slit;† and as the experiment requires no apparatus, I have recommended it in my lectures for the last two years.

The retinal fatigue induced by white light under various conditions forms the subject of a recent paper by Beck.‡

* 'Phil. Trans.,' B, vol. 191 (1899), p. 19.

† 'Phil. Trans.,' B, vol. 191 (1899), p. 8, and Plate I, figs. 4 and 5.

‡ 'Archiv für die ges. Physiologie,' vol. 76, p. 634.

March 1, 1900.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thank ordered for them.

In pursuance of the Statutes, the names of Candidates for electio into the Society were read as follows:—

Adeney, Walter Ernest, D.Sc.
Alcock, Alfred William, Major, I.M.S.
Allen, Alfred Henry, F.C.S.
Ardagh, Sir John, Major-General, R.E.
Ballance, Charles Alfred, F.R.C.S.
Bourne, Gilbert C., M.A.
Bovey, Professor Henry T., M.A.
Boyce, Professor Rubert.
Bridge, Professor Thomas William, M.A.
Brown, Adrian John, F.C.S.
Brown, John.
Bruce, John Mitchell, M.D.
Budge, Ernest A. Wallis, D.Litt.
Burch, George James, M.A.
Callaway, Charles, D.Sc.
Cardew, Philip, Major, R.E.
Clowes, Frank, D.Sc.
Copeman, Sydney Monckton, M.D.
Crookshank, Professor Edgar March, M.B.
Darwin, Horace, M.A.
David, Professor T. W. Edgeworth, B.A.
Dixon, Professor Alfred Cardew, M.A.
Dixon, Professor Augustus Edward, F.C.S.
Dyson, Frank Watson, M.A.
Farmer, John Bretland, M.A.
Feilden, Colonel Henry Wemyss.
Gray, Professor Thomas, B.Sc.
Hamilton, Professor David Jame M.D.
Hardy, William Bate, M.A.
Harmer, Frederic William, F.G.S
Hiern, William Philip, M.A.
Hill, Leonard, M.B.
Hills, Edmond Herbert, Captai R.E.
Hopkinson, Edward, M.A.
Horne, John, F.G.S.
Jackson, Henry Bradwardin Captain, R.N.
Knott, Cargill Gilston, D.Sc.
Letts, Edmund Albert, D.Sc.
Lewis, Sir William Thomas, Bart M.Inst.C.E.
Lister, Joseph Jackson, M.A.
MacArthur, John Stewart, F.C.S.
Macdonald, Hector Munro, M.A.
MacGregor, Professor Jame Gordon, D.Sc.
Maclean, Magnus, D.Sc.
Mallock, Henry Reginald Arnulpl
Mance, Sir Henry C., C.I.E.
Manson, Patrick, M.D.
Marsh, James Ernest, M.A.
Martin, Charles James, M.B.
Mather, Thomas.
Matthey, Edward, F.C.S.
Meyrick, Edward, B.A.

Mill, Hugh Robert, D.Sc.
Muir, Thomas, M.A.
Oliver, John Ryder, Major-General (late R.A.).
Payne, Joseph Frank, M.D.
Perkin, Arthur George.
Rambaut, Professor Arthur A., M.A.
Russell, James Samuel Risien, M.D.
Salomons, Sir David, M.A.
Saunders, Edward.
Schlich, Professor William, C.I.E.
Sell, William James, M.A.
Sidgreaves, Rev. Walter, S.J.
Smith, James Lorrain, M.D.
Smith, Professor William Robert, M.D.
Smithells, Professor Arthur, B.Sc.
Spencer, Professor W. Baldwin, B.A.
Swinburne, James.
Swinton, Alan Archibald Campbell, Assoc. M.Inst.C.E.
Symington, Johnson, M.D.
Tatham, John F. W., F.R.C.P.
Thomas, Michael Rogers Oldfield, F.Z.S.
Ulrich, Professor George Henry Frederic, F.G.S.
Walker, James, M.A.
Walker, Professor James, D.Sc.
Waterhouse, James, Colonel.
Watkin, Colonel, R.A.
Watson, William, B.Sc.
Watts, Philip.
Whetham, William C. D., M.A.
White, William Hale, M.D.
Wilson, Charles T. R., M.A.
Woodhead, Professor German Sims, M.D.
Woodward, Arthur Smith, F.G.S.
Wright, Professor Edward Perceval, M.A.

The following Papers were read:—

I. "An Experimental Inquiry into Scurvy." By F. G. JACKSON and VAUGHAN HARLEY. Communicated by LORD LISTER, P.R.S.

II. "The Velocity of the Ions produced in Gases by Röntgen Rays." By Professor J. ZELENY. Communicated by Professor J. J. THOMSON, F.R.S.

III. "Mathematical Contributions to the Theory of Evolution. VIII. —On the Correlation of Characters not Quantitatively Measurable." By Professor KARL PEARSON, F.R.S.

"Researches on Modern Explosives. Second Communication." By W. MACNAB, F.I.C., and E. RISTORI, Associate M. Inst., C.E., F.R.A.S. Communicated by Professor RAMSAY, F.R.S. Received January 29,—Read February 1, 1900.

In our communication published in the 'Proceedings of the Royal Society,' vol. 56 (in which we gave results of the determination of the calories evolved and analysis of the products of combustion of various

explosives), reference was made to certain experiments we had then begun for the purpose of determining the actual maximum temperature reached during explosion. We have now made a long series of experiments in this direction, and propose to communicate some of the results so far obtained, although the research is not yet complete.

Some experiments have already been made by others for the purpose of determining the temperature during explosion by placing strips of metal of different melting point in a closed bomb and observing the result after firing. Noble and Abel in their well-known communication on explosives found in this way that the temperature produced by the explosion of black gunpowder was slightly above the melting point of platinum.

There have been also several communications, on the same subject made by others, who have deduced the temperature during explosion from theoretical considerations, but these calculations involved assumptions which as yet do not rest on an experimental basis. It appeared to us desirable, therefore, to endeavour to determine experimentally and with greater accuracy than has hitherto been done, the actual temperature developed when an explosive is fired in a closed vessel.

The practical solution of this problem is, however, beset by several difficulties; amongst others, the intensity of the temperature, the extreme shortness of duration of the maximum temperature, and the necessity of conducting the explosive reactions in a closed vessel.

We were led to try a modification of the pyrometric method developed by Sir W. C. Roberts-Austen, by observing that a thin platinum wire used for firing the explosive in the vessel by electricity was often melted by the heat produced by the explosion, while thicker platinum wires, which served to support the capsule containing the explosive, were unaffected.

This showed that the temperature reached was above the melting point of platinum, and also that the duration of the maximum temperature was very short. In the case of the thin wire, the small mass of the metal allows the heat to penetrate it with sufficient rapidity to raise it to the melting point before the period of maximum temperature is past, while with the thick wire the time does not suffice for the larger mass to be heated to the same extent.

These considerations led us to argue that if rhodium-platinum couples of wires of different diameters, sufficiently thick not to be melted during explosion, were used in a bomb, the deflections of the galvanometer indicated would vary inversely with the sizes of the wires forming the couples; that in this way we might get data which would enable us to calculate the deflection of an infinitely thin couple which could be capable of taking up the heat in an infinitely short time, and that this deflection, expressed in degrees, would represent the actual maximum temperature reached. We also expected

that the indication of the galvanometer would show the rapidity with which the temperature rose at the moment of the explosion, as well as the rate at which the cooling took place.

Through the kindness of Sir W. C. Roberts-Austen we were enabled to make some preliminary experiments in his laboratory, connecting the wires from a couple in our bomb with the galvanometer in his photographic-recording apparatus. The results of these experiments were so encouraging that a similar photographic-recording apparatus was procured, only introducing such slight modifications as were required to make it more suitable for our purpose.

We also had a special lid made for the calorimetric bomb previously described in our former communication, this lid being similar to the other, with the exception of two insulated conical pins, one made of pure platinum and the other of platinum alloyed with 10 per cent. of rhodium. These pins were used in the inside of the lid as points of attachment for the thermo-couple, their ends outside the lid being connected with the galvanometer. The couples were made of platinum and rhodium-platinum wires in contact. Several couples of different thicknesses were prepared for us by Johnson and Matthey, fusing the ends of the platinum and rhodium-platinum wires together and then drawing the junction through a die until it had the same diameter as the rest of the wire.

As we shall have to refer to these couples by number in future, we give in Table I the diameters and areas, and the number by which we distinguish each couple. As will be seen, ten different couples were made, the diameters varying from 0·044 to 0·01 of 1 inch :—

Table I.

Couple No.	Diameter in inches.	Area in sq. inches.
1	0·044	0·00152
2	0·040	0·00125
3	0·035	0·00099
4	0·028	0·00061
5	0·026	0·00053
6	0·022	0·00037
7	0·018	0·00025
8	0·015	0·00017
9	0·012	0·00011
10	0·010	0·00008

Fig. 1 shows the explosive bomb with the lid separate, and the arrangement of connections which are made through the lid are clearly seen. Three insulated pins pass through the lid; two of them, as above indicated, are those which connect with the thermo-couple, the other one is used as one of the terminals for the firing wire, the

other terminal being attached to the body of the bomb outside. Thus there are two electric circuits absolutely insulated from each other.

The position of the cup which contains the explosive can be seen in the figure (fig. 1), and the position of the couple in respect to the cup

FIG. 1.

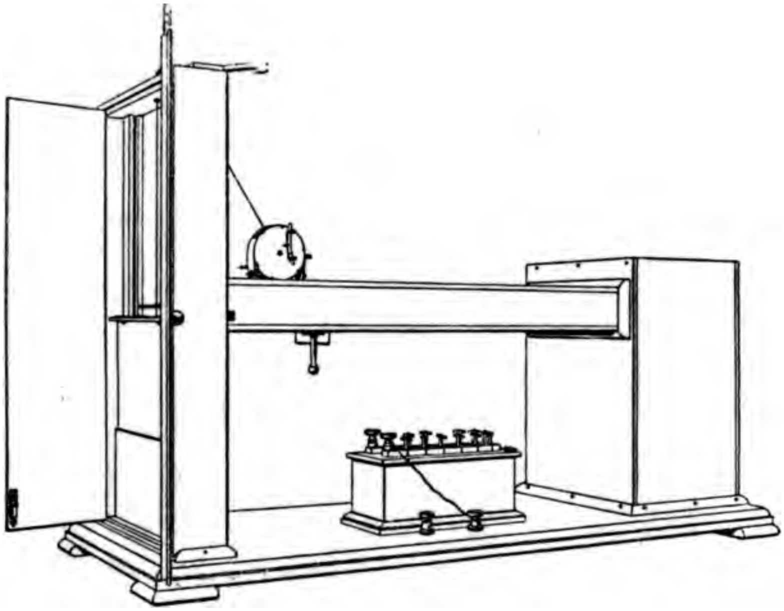
has been varied in a way that will be explained later on. The connections from the firing wire were led to an electric battery, which is set in action when it is required to ignite the explosive.

The connections from the thermo-couple are led to the galvanometer, which is inside the recording apparatus shown in fig. 2. The general details of the apparatus are well known, and have been described by Sir W. C. Roberts-Austen.

Lime-light is used to throw a spot of light on the mirror of the galvanometer, from which it is thrown on to a photographic plate through a horizontal slit, equal in length to the breadth of the plate. This photographic plate is held in a weighted frame, which falls past the slit with a rate of descent uniformly regulated by clock-work, con-

trolled by adjustable vanes. The rate of descent of the plate in the experiments was one inch in 2½ seconds, or equal to four-tenths of an inch for every second of time.

FIG. 2.



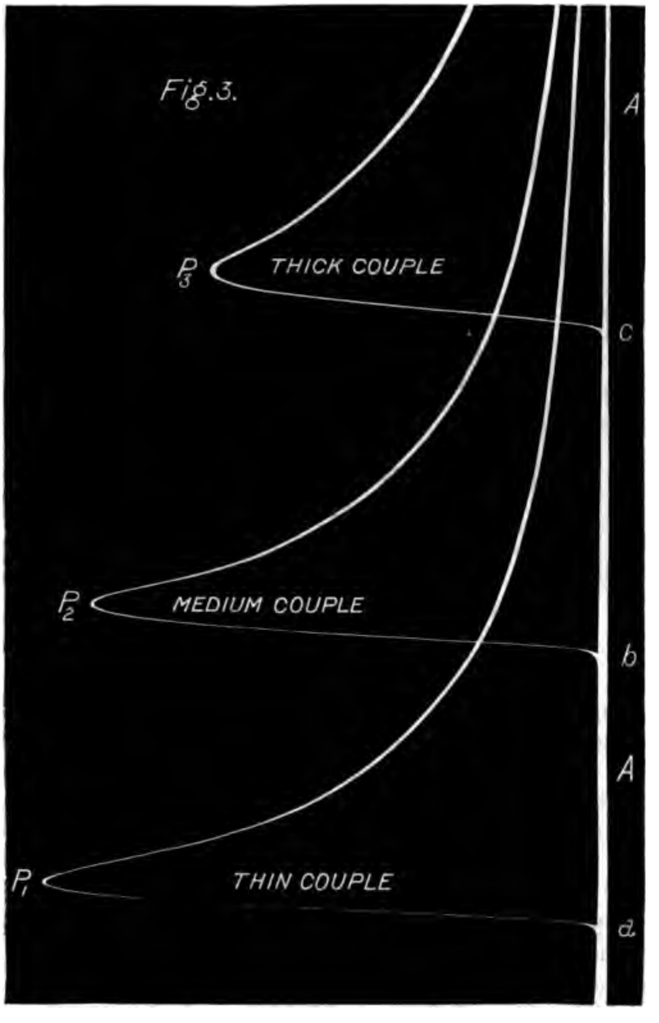
In using the apparatus, a datum line is first traced, and this is done by letting the plate fall with the galvanometer at rest, the circuit being closed, when the spot of light traces a continuous vertical line at one edge of the plate. The plate is then brought back to its original position and re-started on its descent. After it has fallen a short distance, in order to make sure it has acquired a steady rate of motion, the explosive is fired and the thermo-couple in the bomb is in consequence heated, and the current generated deflects the mirror of the galvanometer, and therefore the spot of light, horizontally and proportionately to the temperature attained by the couple; and then, as cooling sets in, the original position of the spot of light is gradually resumed.

The result is recorded as a curve, which shows the combined movements of the spot of light and of the photographic plate.

Fig. 3 shows one of the plates on which three records were successively taken. In these three separate experiments, couples of different thicknesses, but with the same charge and kind of explosive, were used.

AA is the datum line; the point of departure *a* of the spot of light from it shows when the first charge was fired; the spot of light, owing to the deflection of the galvanometer, travelled rapidly to the

FIG. 3.



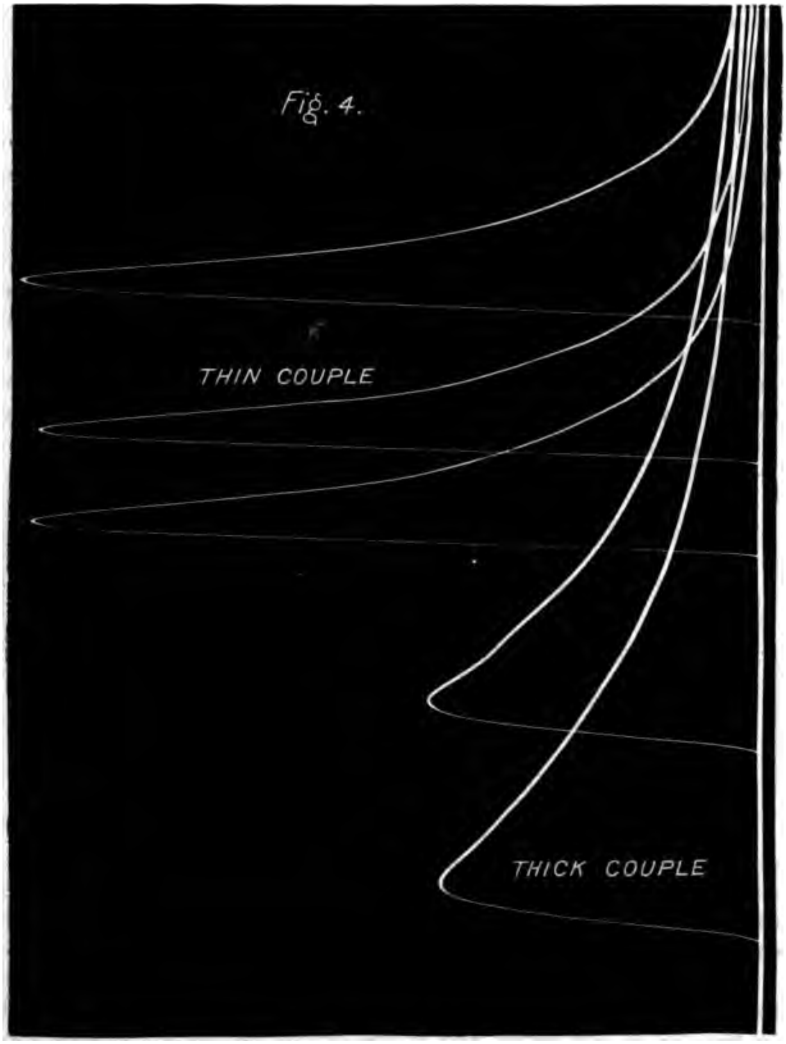
left, until the maximum point P_1 was reached, then more slowly returned, as cooling set in, towards its normal position. *b*, *c*, P_2, P_3, are the corresponding points in the other two experiments.

The maximum deflections, as shown by P_1, P_2, P_3, are seen to be in inverse order to the thickness of the couples used, the greatest deflection being obtained with the thinnest couple.

It is also noticeable that the thinner the couple the sooner is the maximum point reached, *i.e.*, the steeper is the curve and the sharper its point, while with a thicker couple the point is more rounded, and the maximum more slowly reached.

Fig. 4 shows photographs of five results, two with a thick, and three with a very thin couple.

FIG. 4.



Many difficulties were encountered in carrying out these researches in order to secure reasonable accuracy in the work. Several hundred

experiments have been made on the lines above indicated, but a large number of the results have had to be discarded.

To begin with, one point which had to be studied was whether the size of the grain of the explosive would make any difference in the results; but after numerous experiments we have come to the conclusion that, within reasonable limits, the size of the grain exercises no influence under the conditions of these experiments.

In all the experiments about to be described, the explosives used were all gelatinised preparations of gun-cotton alone, or mixed with nitro-glycerine, all in the form of small grains, and the charges were fired in the bomb full of air.

A difficulty observed was that the deflections of the galvanometer were different, everything else being equal, when the position of the couples varied in relation to the position of the explosive. It became necessary, therefore, to carry out a series of experiments in order to determine which position of the couple gave greatest and most uniform results.

The following Table II gives the results of the experiments made to find the hottest place in the bomb. The same charge of the same explosive and the same couple were used in all the experiments, the only difference being that the position of the couple in relation to the explosive which was held in a platinum capsule. Three experiments were made in each position.

Table II.

Couple No. 5.

	Deflection of light in mm.			Mean.
Position of couple bent into capsule in centre of charge	118,	131,	130	126·3
Couple 1″ above explosion	159,	156,	149	154·6
„ 2″ „	156,	163,	153	157·5
„ 3″ „	168,	165,	163	165·5
„ 3¼″ „ (furthest practicable)	160·5,	168·5,	155·4	161·4

These results are shown graphically in Diagram 1, where the vertical distance represents the deflection of the galvanometer, and the horizontal the position of the junction of the couple in relation to the charge. The deflection is least when the junction is embedded in the charge, and greatest when about 3 inches above it.

As will be seen from the diagram, there is very little variation in the results between 2½ inches and 3¼ inches (the maximum distance allowed by the size of the bomb), and as the actual maximum was shown at 3 inches, we have in all future experiments placed the thermo-couple at 3 inches above the surface of the charge.

Another point which we had to consider was the different deflection caused by the firing of different quantities of the same explosive in the same bomb.

DIAGRAM 1.

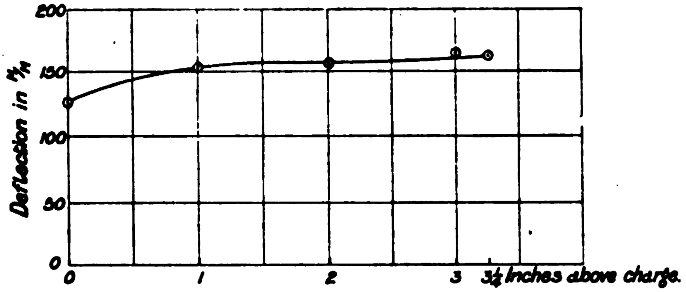
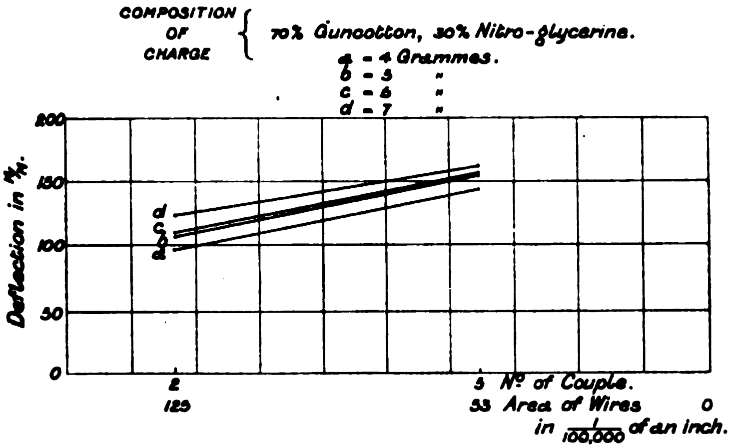


Diagram 2 shows the result of firing 4, 5, 6, and 7 grammes of the same explosive in the same volume with couples 2 and 5.

DIAGRAM 2.



The points of deflection have been connected by a straight line simply for the purpose of showing the general parallelism of these observations.

With charges of less than 4 grammes the results were very irregular, and we have not fired charges larger than 7 grammes, the bomb not being constructed to withstand high pressures.

Having thus ascertained the conditions of working which gave fairly concordant results, a series of experiments was made with couples of different diameters, and, by means of introducing some suitable resistance into the circuit of the galvanometer, it was arranged that the

deflection when the thinnest wire was used should be about the maximum that the photographic plate could record.

The explosive used for this series of experiments was Ardeer ballistite, composed of 70 per cent. gun-cotton and 30 per cent. nitroglycerine, and was in the form of thin square flakes. In each case three experiments were made, and the results are shown in Table III.

Table III.

Charge, 4 grammes Ardeer Ballistite, 70 per cent. Gun-cotton, 30 per cent. Nitro-glycerine.

No. of couple.	Deflection in mm.	Mean.	Maximum.
1	85, 81·5, 83·5	83	85
2	102, 90·5, 98·5	97	102
3	109, 115·5,	112·5	115·5
4	131, 128·5, 138·5	132·5	138·5
5	149, 149, 147	148	149
6	152·5, 158·5, 151	154	158·5
7	161, 170, 166	165·5	170
8	185·5,* 192	189	192

* After the first experiment, the wire was partially fused, but not broken.

It will be seen that the results are fairly uniform, the average variation from the maximum to mean being only between 2 and $2\frac{1}{2}$ per cent.

In the case of couple No. 8, which is only 0·015 inch in diameter, we found that after the first experiment it was partially fused but not broken, thus showing that we had reached the practical limit of fineness of wire for this particular explosive.

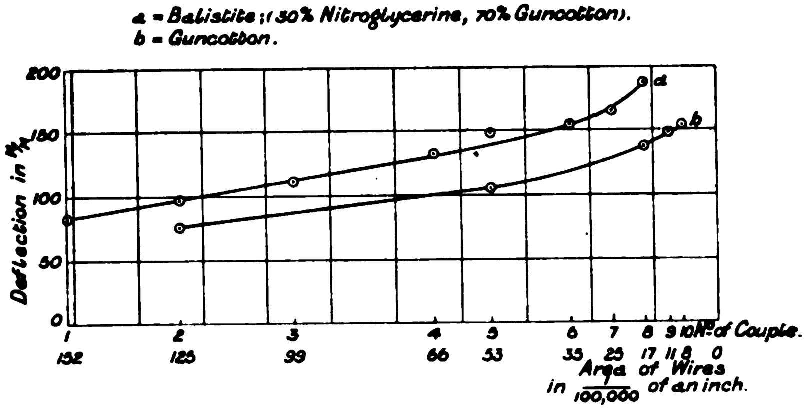
The character of the increase of the deflection in inverse proportion to the diameter of the couple is clearly shown in Diagram 3, where two similar curves are shown for two different explosives.

The curve A shows the results given in Table III, and the curve B the results of a similar series of experiments made with gun-cotton fired with couples 2, 5, 8, 9, and 10.

In the case of gun-cotton, the temperature being so much lower, as clearly shown in the diagram, we have been able to use thinner couples, and there was no fusion up to No. 10, which is 0·01 inch diameter, but a thinner couple (0·005 inch) was fused. The curves have been drawn as nearly as possible following the actual measurements, which are indicated by dots surrounded by circles; as it will be seen, the curves are very regular, and it is particularly noticeable that the curves of the two explosives are very similar in character.

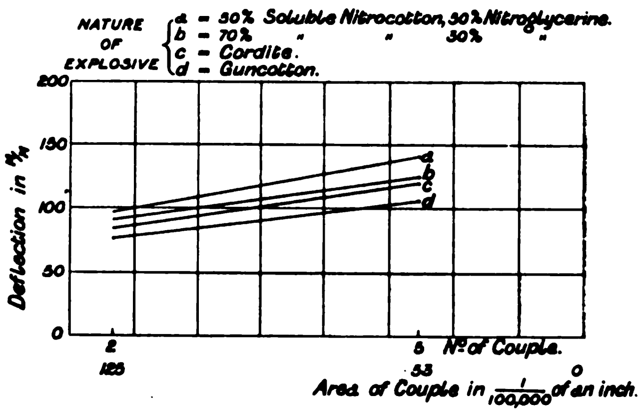
In order to get comparative data for several explosives, we made

DIAGRAM 3.



another series of experiments, the results of which are indicated in Diagram 4.

DIAGRAM 4.



The vertical lines indicate, as before, the maximum deflection of the galvanometer obtained with charges of 4 grammes with couples 2 and 5 for the following explosives: Gun-cotton, cordite, ballistite of 70 per cent. soluble nitro-cotton, and 30 per cent. nitro-glycerine, and ballistite containing 50 per cent. soluble nitro-cotton and 50 per cent. nitro-glycerine.

In the same way as in Diagram 2, each of the points corresponding have been connected by straight lines to show the parallelism of the observations.

It will be seen from the diagram that the larger the proportion of nitro-glycerine in the ballistite the higher is the temperature during

explosion; but, on the other hand, cordite, although it contains as much as 58 per cent. nitro-glycerine, owing to the fact that it contains also vaseline, gives a temperature lower than that of ballistite containing only 30 per cent. nitro-glycerine and no vaseline. Of course, the minimum deflection is the one due to gun-cotton, which contains no nitro-glycerine.

Similar experiments have also been repeated with other explosives and with different charges, and, in every case, the same comparative results have been obtained.

The above refers only to a part of the experiments which have been carried out so far. Another series is now in progress for determining the other necessary elements which will be required before we can accurately express the value of these deflections of the galvanometer in degrees of temperature. One important element which comes into play is the inertia of the galvanometer itself in connection with the shortness of the time during which the maximum temperature exists, and there are also other points which are being investigated, and these will form the subject of a further communication.

We have, however, thought it advisable not to delay communicating the above results, as already the described method shows the possibility first of all of obtaining approximately an idea of the temperature during explosion, and, secondly, it shows a clear way by which the comparative temperatures for various explosives can be determined. These, taken in connection with the results shown in our former communication, will serve, we hope, to give a better knowledge of the different modern explosives which are now commonly used.

"The Spectrum of α Aquilæ." By Sir Norman Lockyer, K.C.B., F.R.S., and A. Fowler. Received January 18,—Read February 8, 1900.

[Plate 1.]

The study of enhanced lines throws considerable light on the spectrum of α Aquilæ, the peculiarities of which were first described by Professor Pickering* and Dr. Scheiner† in 1889. In this spectrum the lines of hydrogen are strong and broad, but the additional lines, instead of being faint and sharp as in most other stars of this class, are faint and diffuse. Dr. Scheiner stated that these apparent bands were identical with the most conspicuous groups of lines in the solar spectrum, and further that this appearance of the spectrum can be

* Third Annual Rep. Henry Draper Memorial, p. 5.

† 'Ast. Nach.,' 2924.

imitated by holding a rather faint drawing of the solar spectrum at such a distance that the individual lines are no longer visible.

From a consideration of the photographs taken at Harvard College Observatory, Professor Pickering suggested in 1891 that the diffuseness of the lines in the spectrum of α Aquilæ and certain other stars was perhaps due to a rapid rotation of the star.* That rotation might be capable of producing such effects had already been suggested by Abney in 1877.†

Photographs taken at Kensington with large dispersion, during 1892, led to the adoption of Professor Pickering's view, and the spectrum of α Aquilæ was classed with that of β Arietis, in which we apparently got the same lines quite sharp.‡

In 1895 Dr. Scheiner again referred to this spectrum,§ and suggested that it represents a transition stage from the first to the second type; as an alternative explanation, he mentions the view that the spectrum may be a composite one, in which a spectrum of the first type is superposed upon one of the second.

In a recent paper‖ Dr. Vogel has discussed the spectrum of α Aquilæ chiefly with reference to its motion in the line of sight, but he also considers the question of the haziness of the lines. He refers to some experimental photographs which depict the solar spectrum with its lines broadened by a cylindrical lens, or by a photograph taken out of focus, and states that spectra of this kind have been obtained in which the close lines run together so as to produce a spectrum resembling that of α Aquilæ. He adds that the exact comparison of the two spectra shows that the agreement is not perfect, in particular that the G group is hardly indicated in the spectrum of α Aquilæ, while it comes out strongly in the solar spectrum when thrown out of focus. He accordingly places the spectrum of α Aquilæ in his Class I*a* 3, of which α Cygni, β Cassiopeiæ, and Procyon are members,¶ and further concludes that the lines are broadened in consequence of rapid rotation, without, however, referring to previous suggestions to the same effect.

The general result is that while Vogel classes α Aquilæ with α Cygni, which, on the meteoritic hypothesis, is a star of increasing temperature, the work at Kensington indicates that it should be classed with stars like β Arietis, which there is every reason to believe to be cooling. This difference as to facts is so important that the whole question has been re-investigated.

* 'Annals Harv. Coll. Obs.,' vol. 26 (1891), Pt. I, p. 21.

† 'Monthly Notices R.A.S.,' vol. 37, p. 278.

‡ 'Phil. Trans.,' A, vol. 184 (1893), p. 697.

§ 'Pub. Ast. Obs. zu Potsdam,' vol. 7, Part II, p. 232.

‖ 'Sitzber. Akad. Berlin,' Nov. 1898; translated in 'Astrophys. Journ.,' Jan. 1899.

¶ 'Astrophys. Journ.,' vol. 2 (1895), p. 346.

The Kensington Photographs.

An investigation of the spectrum of α Aquilæ was commenced a Kensington in 1890, and, with the various instruments employed u to 1892, fifteen negatives were obtained.* In all these the line were ill defined, and it was decided to take a special series of photo graphs "in order to determine whether the haziness of its spectrun lines is invariable."† Since then a considerable number of photo graphs has been obtained, but although variations have been suspecte it is found difficult to establish their reality. One thing seems quit certain, namely, that the lines are always ill defined.

At the Royal Society Conversazione in 1894, enlarged copies o photographs of the spectra of α Aquilæ and β Arietis were exhibite which indicated that Pickering's view that the haziness of the lines i due to rotation is probably correct.

Dr. Scheiner's experiment of photographing the solar spectrum ou of focus has since been repeated; but while it was found possible t produce bands in this way, only a few of them agree with those i α Aquilæ. Among these coincident bands are 4031—4036 (Mn) 4046 (Fe), 4064 (Fe), 4132—4135 (chiefly Fe), 4143·6—4144 (Fe) 4226·9 (Ca), 4250·3—4251 (Fe), 4260·2—4260·6 (Fe), 4271·3—4271· (Fe).

On the other hand, by taking an out-of-focus enlargement of a negative of the spectrum of β Arietis, the violet being put more ou of focus than the blue, the spectrum of α Aquilæ is almost perfectly reproduced (see Plate). The difference in width of the bands appear to be sufficiently explained by the gradually increasing dispersion i prismatic spectra as the violet end is approached. With the instru ment employed at Kensington a tenth-metre near λ 4046 is repre sented by a distance on the photographs about 1·4 times as great a that corresponding to the same difference of wave-length near λ 4384 and since the velocity which would produce a displacement of one tenth metre at λ 4384 would produce a displacement of 0·92 tenth metre at λ 4046, the displacements on the photographs for the same velocity, with the particular instrument employed, will be in the pro portion of 1 to 1·29 at λ 4384 and λ 4046 respectively.

Classification of the Star.

This experiment appears to be a sufficient demonstration of the essential similarity of the spectra of α Aquilæ and β Arietis, so that the former conclusion that the two stars should be classed together is perfectly justified.

* 'Phil. Trans.,' A, vol. 184 (1893), pp. 685–688.

† *Ibid.*, p. 696.

According to the earlier work at Kensington, stars like β Arietis were classed in Group Vα,* that is, between stars like Sirius and those like Procyon. The work on enhanced lines which has been done since then enables us to carry on the work of classification with much greater precision, since we have now a means of estimating relative temperatures with considerable accuracy. In this way we learn that stars at each stage of temperature fall into two groups, one of which represents stars of increasing temperature, and the other including stars of decreasing temperature. α Aquilæ and β Arietis fall in the latter group, and are to be regarded therefore as stars in which photospheres have formed. The later work on the classification of spectra has shown that it is sufficient for all practical purposes to include both in the Sirian group of stars.

This question of classification is further elucidated by a more detailed examination of the spectrun of α Aquilæ in relation to α Cygni and the Sirian stars. The foregoing demonstration of the likeness between α Aquilæ and β Arietis leads us to expect that the origins of the lines in the spectrum of α Aquilæ will be the same in the main as those of β Arietis and Sirius. In these stars the temperature of the absorbing vapours is intermediate between that of the arc and that at which enhanced lines appear alone, so that the spectra show both arc and enhanced lines. The origins of the chief enhanced lines in the spectrum of Sirius have already been investigated,† and practically the same lines occur in β Arietis. Besides these enhanced lines there are several well-known arc lines, such as the iron triplets and the blue line of calcium, which can be readily identified. The origins of some of the lines of both classes are shown in the plate which accompanies this paper, enhanced lines being shown at the bottom and arc lines at the top.

It will be seen that enhanced lines of iron appear in α Aquilæ, but have not the same relative intensity as in α Cygni; the most enhanced line of iron (λ 4233·3), for example, which in α Cygni is represented by a very strong and well-defined line, is in α Aquilæ very weak and hazy. On the other hand, some of the enhanced lines of iron less refrangible than Hγ are fairly prominent. The principal enhanced lines of magnesium, strontium, and titanium are also certainly present, as shown in the plate. The enhanced double line of silicium at $\lambda\lambda$ 4128·1, 4131·1, if present, is very weak, a moderately strong hazy line, rather less refrangible than the silicium double, making it rather difficult to determine whether the latter is certainly present.

Among the arc lines present are those of the iron triplet in the violet ($\lambda\lambda$ 4045·90, 4063·76, 4071·79), which are clearly seen, but the iron triplet in the blue ($\lambda\lambda$ 4383·70—4415·27) cannot be identified

* 'Phil. Trans.,' A, vol. 184 (1893), p. 726.

† 'Roy. Soc. Proc.,' vol. 65 (1899), Plate 7.

with certainty. The place occupied by the manganese quartet ($\lambda\lambda$ 4030·88—4035·88) is covered in the spectrum of the star by what appears to be a broad hazy line, which is probably composed of the individual components of the quartet merged together. The arc line of calcium at λ 4226·90 is one of the most prominent lines in the spectrum.

The classification of the spectrum of α Aquilæ may therefore be considered as settled; it does not sufficiently resemble α Cygni to justify Vogel's view that it should be classed with that type of star, while, on the other hand, apart from the haziness of the lines, it does bear a very strong resemblance to β Arietis and other Sirian stars, and should therefore be classed with them.

[*Note, February* 8.—In a later publication* Vogel places α Cygni in his Class I*a* 2, with Sirius, β Arietis, &c., but this does not materially modify the conclusions arrived at.]

There are other points on which this demonstration of the similarity of α Aquilæ and β Arietis may be brought to bear, among them being the determination of the lines most suitable for the measurement of the velocity of the star in the line of sight, and the approximate determination of the velocity of rotation necessary to produce the observed haziness of the lines.

Lines suitable for the Determination of the Velocity of the Star in the Line of Sight.

For the measurement of the velocity of α Aquilæ, Deslandres has employed comparison spectra of hydrogen, iron, and calcium.† Vogel, however, questions the advantage of using the spectra of iron and calcium as comparisons for this purpose, on the ground that "the lines in the spectrum of α Aquilæ are so diffuse . . . . that between H_β and H_δ no lines except those of hydrogen and the magnesium line at λ 4481 can be identified with known lines." He himself has used the H_γ line alone as a term of comparison, and concludes that there are no indications of a periodic change in the velocity of the star in the line of sight, such as was supposed by Deslandres.

We now know with certainty the origins of a considerable number of the lines in α Aquilæ, so that measurements of the velocity of the star are placed on a surer basis. Since the spectrum of α Aquilæ is simpler than that of the sun, some of the broad lines do not represent confused groups of lines, but are broadened individual lines. The latter class of lines, when of known origin, seems to be well adapted for the

* 'Pub. Ast. Obs. Potsdam,' 1899, vol. 12, Part I, p. 49.

† 'Comptes Rendus,' vol. 121 (1895), p. 629.


ENHA
LIN
G
FE
TI

measurement of the velocity of the star, for though they may be still somewhat wide, they are much less so than the lines of hydrogen. Among these broadened individual lines are :—

Iron (enhanced lines).—4178·95, 4233·25, 4385·55, 4549·64.
„ (arc lines).—4045·90.
Titanium (enhanced lines).—4417·98, 4443·98.
Strontium.—4215·66.

The enhanced line of magnesium at 4481·3 is usually sharply defined in stellar spectra, but the fact that it is generally fluffy in the comparison spark disqualifies it for accurate measurements.

The Velocity of Rotation.

Assuming that β Arietis represents the spectrum of α Aquilæ as it would appear if the axis were directed towards the earth, we can get a general idea of the velocity of rotation necessary to produce the observed broadening of the lines. For this purpose lines which occur in groups are obviously unsuitable, but we can utilise the lines to which attention has just been drawn. Taking the enhanced line of iron at λ 4178·95, we find that its thickness is increased from about two to four tenth-metres, and this corresponds to a surface velocity of the star at the equator of about 45 miles per second, supposing that the axis is perpendicular to the line of sight. Similar measurements of the broadening of the magnesium line 4481·3 yield a velocity of about 40 miles per second. Since only a small portion of the surface of the star could exhibit the effects of the maximum velocity, it is probable that these values are too low, really representing the equatorial velocity of rotation resolved along the line of sight with reference to a point somewhere between the limb and centre of the star.

Dr. Vogel gives reasons in his paper for supposing the velocity of rotation to be possibly 27 kilometres (16·8 miles) per second, but this determination does not depend upon measurements of individual lines.

General Conclusions.

The investigation of the Kensington photographs of the spectrum of α Aquilæ has thus led to the following conclusions :—

(1) Apart from the general haziness of the lines, the spectrum presents no unfamiliar features.

(2) The spectrum is of the Sirian type, showing enhanced lines of various metals, and a smaller number of arc lines.

(3) A rapid rotation of the star, as first suggested by Pickering, appears to be a simple and sufficient explanation of the peculiarities of the spectrum.

DESCRIPTION OF PLATE.

A. Solar spectrum, purposely out of focus.
B. α Aquilæ.
C. β Arietis, purposely out of focus.
D. β Arietis, in focus.

The photographs of the spectrum of α Aquilæ which have been obtained at Kensington since 1890 were nearly all taken by Messrs. Fowler, Baxandall Shackleton, and North. Mr. Baxandall has assisted in the determination of origins.

The photographic plate has been prepared from the original negatives by Sapper Wilkie, R.E.

"The Velocity of the Ions produced in Gases by Röntgen Rays.' By JOHN ZELENY, B.Sc., B.A., Assistant Professor of Physics University of Minnesota. Communicated by Professor J. J THOMSON, F.R.S. Received February 15,—Read March 1 1900.

(Abstract.)

The sum of the velocities with which the positive and the negative ions that are produced in gases by the Röntgen rays move when in a unit electric field has already been determined by an indirect method by E. Rutherford.* In the experiments here described the velocity was determined in a number of gases for the positive and negative ions separately, by comparing the ionic velocity directly with that of a stream of gas. The stream of gas was made to flow between two concentric cylinders, which were maintained at different potentials By passing a narrow beam of Röntgen rays through the cylinders at right angles to their length, a narrow layer of ionised gas was produced. Due to the electric field between the two cylinders, the ions of this layer tended to move radially towards, or away from, the axis of the cylinders, but at the same time they were carried along by the stream of gas. Of the ions of this layer which travelled inwards those that started from the inner surface of the outer cylinder were carried a distance X by the gas stream before they reached the surface of the inner cylinder.

This distance is dependent directly upon the mean velocity of the gas stream, and inversely upon the difference of potential between the two cylinders. For obtaining the difference of potential which must be used to allow the ions to be carried a certain distance along the tubes by the gas stream the inner cylinder was divided at some distance from the beam of rays into two parts, insulated from each

* E. Rutherford, 'Phil. Mag.,' November, 1897.

other. That one of these parts which was not traversed by the rays was connected to a pair of quadrants of an electrometer, so that it was possible to tell when any ions reached it. A series of readings was taken for the charge reaching the electrometer in a given time for different values of the potential of the outer cylinder. From this was determined the value of this potential for which the ions starting from the outer edge of the ionised layer were just able to reach the juncture in the inner cylinder.

The ionic velocity in a unit electric field is given by the equation—

$$v = \frac{U(b^2 - a^2)}{2AX} \log_e \frac{b}{a},$$

where U is the mean velocity of the gas stream between the cylinders,
b is the inner radius of the outer cylinder,
a is the outer radius of the inner cylinder,
A is the potential of the outer cylinder, corresponding to
X the distance defined above.

To avoid the presence of vortices in the gas at the place where it was exposed to the rays, a sufficiently small velocity was used, and the gas was previously passed through a long portion of the cylinder to allow the motion to assume a steady state.

The disturbing influence upon the electric field between the cylinders of the free charges formed in the gas during the conduction was diminished by using weak rays. The fall of potential at the electrodes* was also reduced by this means. For diminishing the amount of ionisation due to the secondary radiation produced at the metal surface,† the cylinders were made of aluminium, for which metal the effect is the least.

The spreading of the ions due to diffusion produces an error, the amount of which increases with the time required for the ions to travel between the two cylinders.

The value of this time is found from the equation $T = X/U$, where X and U have the same significance as above.

The experimental values obtained for the velocity decreased as T increased, and from a series of results with different values of T the velocity could be obtained corresponding to $T = 0$. Since in that case the effects of diffusion and similar causes disappear, this result was taken as the desired value of the ionic velocity.

For testing the accuracy of the method, in addition to using different values of U and X, changes were also made in the intensity of the rays, in the diameter of the internal cylinder, and in the metal which formed the inner surface of the outer cylinder.

Determinations were made with the gases when dry and when satu

* J. Zeleny, 'Camb. Phil. Soc. Proc.,' vol. 10, Part I, p. 17.

† J. Perrin. 'Comptes Rendus,' vol. 124, p. 455.

rated with aqueous vapour, as the results were found to be differen in the two cases. This is in agreement with the effect of moistur upon the coefficients of diffusion of the ions, as observed by J. S Townsend.* A summary of the results obtained is given in the follow ing table. The results are reduced to a pressure of 76 cm. of mercury but are not corrected for temperature, the effect of which is nc known.

Ionic Velocities.

Gas.	Velocity in centimetres per second in a field of 1 volt per centimetre.		Velocity in centimetres per second in a field of 1 E.S.U. per centimetre.		Ratio of negative to positive.	Tempera ture.
	Positive.	Negative.	Positive.	Negative.		
Air, dry	1·36	1·87	408	561	1·375	13·5° C
Air, moist	1·37	1·51	411	453	1·100	14
Oxygen, dry	1·36	1·80	408	540	1·320	17
Oxygen, moist	1·29	1·52	387	456	1·180	16
Carbonic acid, dry	0·76	0·81	228	243	1·070	17·5
Carbonic acid, moist	0·82	0·75	246	225	0·915	17
Hydrogen, dry	6·70	7·95	2010	2385	1·190	20
Hydrogen, moist	5·30	5·60	1590	1680	1·050	20

It is believed that in no case is the error greater than 5 per cen while most of the observations indicate a considerably greater accu racy. It is observed that the presence of moisture always diminishe the velocity of the negative ions, and that in carbonic acid the velocit of the positive ions is at the same time markedly increased. Th velocity of the negative ions is the greater in all of the cases excep for moist carbonic acid. The ratios of the velocities of the ions pr viously determined for these gases by the writer† were between thos given above for the dry and for the moist gases, as the influence o moisture was unknown at that time, and the gases had not been drie

E. Rutherford‡ does not state whether he used dry gases in dete mining the sum of the velocities of the two ions produced by Röntge rays; but his result for air (3·2 cm. per second) agrees with the sum o the values separately obtained above for the two ions in dry ai while his values for oxygen (2·8 cm. per second) and hydroge (10·4 cm. per second) correspond to those for the moist gases. H value for carbonic acid (2·15 cm. per second) is higher than thos

* J. S. Townsend, 'Phil. Trans.,' A, vol. 193, 1899.

† J. Zeleny, 'Phil. Mag.,' July, 1898.

‡ E. Rutherford, 'Phil. Mag.,' November, 1897.

here obtained. The value obtained by E. Rutherford* for the velocity of the negative ions produced in dry carbonic acid (0·78 cm. per second) by the action of ultra-violet light, is quite near to that here obtained (0·81 cm. per second) for the ions produced by Röntgen rays, but his values for dry air (1·4 cm. per second) and dry hydrogen (3·9 cm. per second) are considerably smaller.

In discharge from points, A. P. Chattock† has obtained for the velocities of the positive and negative ions in dry air 413 and 540 cm. per second respectively for a field of 1 E.S.U. per cm., which values are quite close to those obtained here for the ions produced by Röntgen rays.

J. S. Townsend‡ has shown that from the coefficients of diffusion of the ions and from their velocities it is possible to compare the charges carried by the different ions, and also to compare them with those carried by the ions in the electrolysis of liquids. By using the velocities given above with the coefficients of diffusion determined by J. S. Townsend, the values of N*e* are obtained, N being the number of molecules in 1 c.c. of the gas and *e* the charge carried by each ion. The results thus obtained for the moist gases, air, oxygen, and hydrogen, perhaps justify the statement that the charges carried by the positive and the negative ions are equal, and that the charge is the same for the different gases, and is equal to the charge carried by the hydrogen ion in the electrolysis of liquids.

The values of N*e* obtained for the positive ions in these gases when dry are considerably larger than the above, while in carbonic acid all of the results are over 20 per cent. smaller.

"Mathematical Contributions to the Theory of Evolution. VIII. On the Correlation of Characters not Quantitatively Measurable." By Karl Pearson, F.R.S. Received February 7, —Read March 1, 1900.

(From the Department of Applied Mathematics, University College, London.)

(Abstract.)

1. In August last I presented to the Society a memoir on the inheritance of coat-colour in thoroughbred horses, and of eye-colour in man. This memoir, which was read in November of last year, presented the novel feature of determining correlation between characters which were not capable *à priori* of being quantitatively measured. The theoretical

* E. Rutherford, 'Camb. Phil. Soc. Proc.,' vol. 9, Part VIII.
† A. P. Chattock, 'Phil. Mag.,' November, 1899.
‡ J. S. Townsend, 'Phil. Trans.,' A, vol. 193, 1899.

part of that memoir was somewhat brief, but I showed by illustrations that the method could be extended to deal with problems like the effectiveness of vaccination and of the antitoxin treatment in diphtheria. More recently, in studying the phenomena of reversion in Basset Hounds, Mr. Bramley-Moore indicated to me how my method, although correct in theory, differed sensibly in the numerical results with the processes of interpolating employed. I then proposed a new method, and the analytical discussion of its details was worked out in part by Mr. Bramley-Moore himself, by Mr. L. N. G. Filon, M.A., and by myself. Dr. Alice Lee also came to our assistance, and the result is the present joint paper. On the basis of the new methods, we have already worked out upwards of sixty coefficients of correlation, principally of heredity. Thus the thirty-six coefficients of heredity for coat-colour in horses and eye-colour in man have been re-calculated, as well as twelve coefficients for heredity in coat-colour of Basset Hounds given in a paper on the Law of Reversion presented on December 28th, and about to appear in the 'Proceedings.' The great growth of the theoretical investigations has, however, compelled me to break up the old memoir* of last August into two parts, the one (the present) dealing only with theory, and the other with its application to inheritance in the horse and man.

2. The theory of the present memoir depends upon a very simple feature of normal correlation. If $z\delta x_1 \delta x_2 \ldots \delta x_n$ be the frequency of a complex of characters lying between x_1 and $x_1 + \delta x_1$, x_2 and $x_2 + \delta x_2 \ldots$, x_n and $x_n + \delta x_n$, where x_p is the deviation of the pth character from its mean, then

$$\frac{dz}{dr_{pq}} = \frac{d^2 z}{dx_p \, dx_q},$$

where r_{pq} is the correlation of the pth and qth organs.

This simple differential relation enables us to expand z for any number of characters in powers of the correlation coefficients (necessarily less than unity) by Maclaurin's theorem. But since we may replace a differential with regard to a coefficient of correlation by a double differential with regard to the corresponding organs, the coefficients of correlation may be put zero *before* instead of after the differentiation. In other words, we obtain a symbolic operator which, applied to a normal surface of frequency for n-uncorrelated organs, converts it into a correlated surface of frequency with $\frac{1}{2}n(n-1)$ coefficients of correlation of arbitrary values. This operator gives us by aid of certain symbolic equations the expansion of the n-fold integral

$$\int_{h_1}^{\infty} \int_{h_2}^{\infty} \int_{h_3}^{\infty} \ldots\ldots \int_{h_n}^{\infty} z \, dx_1 \, dx_2 \, dx_3 \ldots\ldots dx_n$$

* That memoir was at my own request returned for revision after being accepted for the 'Philosophical Transactions.'

in terms of the $\frac{1}{2}n(n-1)$ coefficients of correlation, and a series of new functions which we term the v-functions. These satisfy the difference equation:

$$v_n = xv_{n-1} - (n-1)v_{n-2}$$

and the differential equation

$$\frac{dv_n}{dx} = nv_{n-1}.$$

In fact

$$v_n = x^n - \frac{n(n-1)}{2\underline{|1}} x^{n-2} + \frac{n(n-1)(n-2)(n-3)}{2^2\underline{|2}} x^{n-4}$$
$$+ (-1)^r \frac{n(n-1)\ldots\ldots(n-2r+1)}{2^n\underline{|r}} x^{n-2r} + \ldots\ldots$$

The calculation of these functions is shown to be easy, and their properties are investigated. In this manner the volume of a frequency surface of the nth order cut off by n planes parallel to the n co-ordinate planes is shown to be capable of calculation, and its value is determined in the numerical illustrations given for example of 1, 2, 3 up to 6-fold correlation. It may be noted that by putting $z_1 = z_2 = z_3 = \ldots\ldots = z_n$, we have really obtained a result which enables us to find the "area" of a "spherical triangle" in n-fold hyperspace in terms of a series ascending by powers and products of the cosines of the angles between its faces.

The application of these results to the correlation of characters not quantitatively measurable, arises from the fact that the n-fold integral above given, and which we have shown how to evalute, measures the total frequency beyond certain boundaries. We can observe, for example, whether horses' coats are bay or darker (or chestnut or lighter), whether eyes are grey or lighter (or, dark grey or darker). Thus by forming mass frequencies instead of frequency distributions for small changes of character, we can find equations to determine the correlation. The probable error of such correlation, the convergency of the series, and other points are investigated.

3. Some discusssion is given to the problem of association, and coefficients allied to Mr. Yule's coefficient of association but somewhat closer in value to the coefficient of correlation are considered, and their relative closeness measured.

4. A number of illustrations of the new method are given from heredity in horses, dogs, and man, and it is shown how normality of frequency must even for such a character as stature* only be looked upon as a first approximation.

An investigation is also made into the influence of superior stock

* Cited by so many as an example of "normality."

in producing superior offspring. It is shown, for example, that if an individual who possesses a degree of character only found in one in twenty be considered "exceptional," then eighteen times as many exceptional men will be born of non-exceptional parents as of exceptional parents; but on the other hand, exceptional parents produce exceptional offspring at a rate ten times as great as non-exceptional parents, the greater gross product of the latter being due to their much greater numbers. In other words, distinguished parents are more likely to have distinguished offspring than undistinguished—ten times as likely—and yet only one distinguished man in nineteen will be born of distinguished parents. The importance of such conceptions for both natural and artificial breeding can hardly be over-estimated.

March 8, 1900.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Bakerian Lecture, "The Specific Heat of Metals and the Relation of Specific Heat to Atomic Weight," was delivered by Professor W. A. TILDEN, F.R.S.

BAKERIAN LECTURE.—"On the Specific Heat of Metals and the Relation of Specific Heat to Atomic Weight." By W. A. TILDEN, D.Sc., F.R.S., Professor of Chemistry in the Royal College of Science, London. With an Appendix by Professor JOHN PERRY, F.R.S. Received February 9,—Read March 8, 1900.

(Abstract.)

The experiments described in this paper were begun with the object of assisting in the determination of the relative values of the atomic weights of cobalt and nickel, but were continued with the further purpose of testing the validity of the law of Dulong and Petit.

The metals cobalt and nickel closely resemble each other in density, melting point, and other physical properties, as well as in atomic weight. The pure metals were prepared for the purposes of these experiments with the most scrupulous care, the cobalt by taking

advantage of the slight solubility of purpureo-cobaltamine hydrochloride in strongly acid solutions, and the nickel by deposition from the carbonyl compound and subsequent solution of the metal and electro deposition. Both were fused by means of an oxyhydrogen flame, and afterwards shaped into bars. For the estimations of specific heat between 15° C. and 100° C. the differential steam calorimeter of Professor Joly was employed.

The mean specific heat of cobalt within these limits of temperature was found to be 0·10303.

The mean specific heat of nickel was found to be 0·10842.

In order further to test the method and the conclusion from the case of cobalt and nickel, gold was compared with platinum, and copper with iron.

The following are the mean specific heats for these pure metals after fusion, and within the same limits of temperature:—

Gold	0·03035
Platinum	0·03147
Copper	0·09232
Iron	0·10983

When these values for the specific heats are multiplied by the respective atomic weights of the several metals, the products are not constant, as the law of Dulong and Petit would seem to require if applicable at all temperatures.

The influence of impurities on the specific heat of several metals was then investigated, and a number of results are given, from which it appears that small quantities of carbon or other non-metallic element tend to increase the specific heat appreciably, while the presence of a small quantity of a foreign metal seems to produce little effect.

A series of calorimetric experiments were next made by the method of mixtures on the two pure metals cobalt and nickel, at the temperature of solid carbon dioxide, −78·4°, and at that of boiling oxygen, −182·5°.

The results, which are given below, show that as the temperature is reduced the value for nickel declines more rapidly than that for cobalt, and hence that, when the mean results are plotted out, the curves steadily approach each other.

The mean specific heats of cobalt and nickel now stand as follows:—

Temperature.	Cobalt.	Nickel.
From 100° to 15°	0·10303	0·10842
„ 15° „ −78·4°	0·0939	0·0975
„ 15° „ −182·4° ...	0·0822	0·0838

and by calculation from the last two results from −78·4 to −182·4°, 0·0712, 0·0719.

It seems probable that at absolute zero the values of the products of specific heat, multiplied by atomic weight, would be identical, or differ only by the very small amount due to experimental error.*

Appendix.

The following calculations have been made by E. R. Verity, Assoc. R.C.S., and H. L. Mann, Assoc. R.C.S. :—

If cobalt and nickel were in the states of perfect gases, their specific heats at constant volume would be

$$k_0 = 0{\cdot}04123 \text{ for cobalt.}$$

$$k_0 = 0{\cdot}04145 \text{ for nickel.}$$

This is on the assumption that k_0 multiplied by the atomic weight is the same as for hydrogen. It is not unreasonable to assume that in no state can the substance have a smaller specific heat than k_0. It is easy to show that for any of the metals the specific heat at atmospheric pressure K is not more than 2 per cent. different from the specific heat under any other condition, such as great hydrostatic pressure, to keep its volume constant. Our ignorance of the molecular state of a solid is so great, that we cannot even speculate on how it is that when 1 gramme of cobalt (we have much the same figures for other metals) at 50° C. rises in temperature to 51° C., whether it is allowed to expand freely or is subjected to great hydrostatic pressure which prevents expansion, the energy 0·041 enters it as what may be called the real sensible heat, and the energy 0·062 enters it as some kind of energy of disgregation, necessary because of change of temperature, and having nothing to do with change of volume or pressure. The facts are not explainable by assuming that the atomic weights are wrong, because, as we see from cobalt and nickel, K approaches the value k_0 at low temperatures. Indeed, if the following formula is correctly deduced from the above measurements, we may say that in the solid state the product of the atomic weight and specific heat may be anything between what it is for hydrogen and 2·7 times this amount. The formula is

$$K = k_0 + \frac{bt^3}{1 + ct^3},$$

where t is the absolute temperature.

* *Note added March* 3.—Further experiments made, since the date of communication, upon the metals silver, copper, iron, and aluminium show, however, that this suggestion will not be realised. The mean specific heat of silver between 15° and 182·4°, for example, is 0·0519, while from 100° to 15° it is 0·0558. The decrease of specific heat at the lower temperature is therefore much less than in the case of cobalt and nickel.

	Cobalt.	Nickel.
k_0.	0·0412	0·0415
b	$1·764 \times 10^{-8}$	$1·764 \times 10^{-8}$
c	$2·55 \times 10^{-7}$	$2·35 \times 10^{-7}$

This formula must give fairly correct values of K from $-180°$ C. to 100° C.

March 15, 1900.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Total Eclipse of the Sun, January 22, 1898. Observations at Viziadrug." By Sir NORMAN LOCKYER, K.C.B., F.R.S., Captain CHISHOLM-BATTEN, R.N., and Professor PEDLER, F.R.S.

II. "A Comparative Crystallographical Study of the Double Selenates of the Series $R_2M(SeO_4)_2,6H_2O$. Part I.—Salts in which M is Zinc." By A. E. TUTTON, F.R.S.

III. "The Theory of the Double Gamma Function." By E. W. BARNES. Communicated by Professor FORSYTH, F.R.S.

"Total Eclipse of the Sun, January 22, 1898. Observations at Viziadrug." By Sir NORMAN LOCKYER, K.C.B., F.R.S., Captain CHISHOLM-BATTEN, R.N., and Professor A. PEDLER, F.R.S. Received January 25,—Read February 22, 1900.

(Abstract.)

The paper is in three parts. The first, by Sir Norman Lockyer, gives an account of the arrangements made for the observations and the general conditions of the eclipse; but the results obtained with the

prismatic cameras are not included, for the reason that the reduction of the photographs is not yet completed.

The second part, by Captain Chisholm-Batten, R.N., gives a full account of the observations made by the officers and men of H.M.S. "Melpomene"; four photographs of the corona, taken with different exposures, accompany the paper.

In the third part, Professor Pedler gives details of the spectroscopic observations which he made by eye with a 6-inch short focus lens and a grating spectroscope.

A general idea of the work at Viziadrug has already been given in a preliminary report.*

"A Comparative Crystallographical Study of the Double Selenates of the Series $R_2M(SeO_4)_2,6H_2O$.—Part I. Salts in which M is Zinc." By A. E. Tutton, B.Sc., F.R.S. Received March 5, —Read March 15, 1900.

(Abstract.)

In this communication are presented the results of the investigation of the group of salts of the above series in which M is represented by zinc, R being represented by potassium, rubidium, and cæsium. The investigation is similar to that which has previously been carried out for the double sulphates of the analogous series.† The work consists of very large numbers of measurements of the exterior angles of the crystals, determinations of density, refractive index, optic axial angle, orientation of optical ellipsoid, and effect of change of temperature on the optical properties, together with the calculation of all the morphological and physical constants derivable from the measurements. The main results are as follows:—

The morphological axial angle of the rubidium zinc salt is approximately the mean of the axial angles of potassium zinc and cæsium zinc selenate.

In the cases of thirty-three out of thirty-six angles between the exterior faces, the value for the rubidium zinc salt is intermediate between the values for the other two salts, and the exceptions are only apparent, being due to changes of opposite sign in adjacent angles following the rule.

The morphological axial ratios for rubidium zinc selenate are intermediate between the ratios of potassium zinc and cæsium zinc selenate.

The common habit of the crystals of the rubidium zinc salt is of an

* 'Roy. Soc. Proc.,' vol. 64, p. 27.

† 'Journ. Chem. Soc., Trans.,' 1893, 337, and 1896, 344.

intermediate character to the habits of the crystals of the potassium zinc and cæsium zinc salts.

An increase of density accompanies a rise in the atomic weight of the alkali metal, and it is greater for the replacement of potassium by rubidium than for that of the latter by cæsium in the proportion of 5 : 4.

The molecular volumes show a similar progression in the order of the atomic weights of the alkali metals, but the replacement of rubidium by cæsium is marked by the greater change.

The distance ratios (topic axes) indicate an extension of the distance separating the structural units in all three axial directions, the maximum being along the symmetry axis.

All these rules relating to the exterior morphology of the crystals are precisely analogous to those previously shown to apply to the double sulphates.

The optical ellipsoid (indicatrix) is found to rotate about the symmetry axis, when the atomic weight of the alkali metal is raised. The amount of rotation is twice as great when cæsium replaces rubidium as when the latter replaces potassium.

The mean refractive index (mean of all three indices) of the rubidium zinc salt is intermediate between the mean indices of the other two salts.

The greatest change accompanies the replacement of rubidium by cæsium.

The double refraction diminishes at an increasing rate as the atomic weight of the alkali metal increases.

It follows from the above that the axial ratios of the optical indicatrix for the rubidium zinc salt are intermediate between those of the other two salts, the second replacement being accompanied by the greater change.

The optic axial angle of the rubidium zinc salt is almost exactly the mean of the widely different optic axial angles of the potassium zinc and cæsium zinc salts. The optic axes have a common plane, and the bisectrices are similarly situated, subject to the rotation of the whole ellipsoid already specified.

The optic axial angles show a progressive change on heating the section plates—namely, a slight increase in the case of the potassium zinc salt, a slight decrease in the case of the cæsium zinc salt, and an almost complete indifference to change on the part of the rubidium salt.

The whole of the specific and molecular optical constants of rubidium zinc selenate are intermediate between those of the potassium and cæsium zinc salts. The molecular refraction and dispersion increase at an accelerating rate with the rise of atomic weight of the alkali metal.

On instituting a comparison between the results now communicated and those formerly published for the triplet of zinc double sulphates, it is found that the replacement of sulphur by selenium is generally accompanied by a change in the morphological and physical constants similar to that which accompanies the replacement of one alkali metal by another of higher atomic weight. The changes due to the latter chemical change are often smaller in the selenate series than in the sulphate series, the greater weight of the initial molecule appearing to offer greater resistance to change. The intermediate character of the constants of the rubidium salt is, however, the invariable rule in both cases.

"An Experimental Inquiry into Scurvy."* By FREDERICK G. JACKSON and VAUGHAN HARLEY, M.D. Communicated by LORD LISTER, P.R.S. Received February 15,—Read March 1, 1900.

(From the Department of Pathological Chemistry, University College, London.)

The view that scurvy is caused by the want of fresh vegetables or lime juice, which has been the teaching of physicians and scientists in past years, would appear to require modification.

In the early part of this century, through the efforts largely of Lind, the better feeding of sailors led to the gradual disappearance of scurvy in the naval service, and from this and other observed facts it was conceived that the disease developed whenever individuals did not receive a sufficient quantity of fresh vegetables, or some substitute, such as lime juice, in the diet.

Garrod held that the cause of scurvy was a deficiency of potassium salts, while others believed the essential factor to be the absence of organic salts, which are present in fruits and vegetables.

Ralfe believed the absence from the food of malates, citrates, and lactates reduced the alkalinity of the blood, and thus was the cause of scurvy. It was proved, however, by analysis that the alkalinity of the blood was not diminished, and the majority of evidence showed no diminution in the quantity of potash salts in the scorbutic blood, so that these explanations had to be abandoned.

Neale, in an article on "Scurvy in the Arctic Regions," published in the 'Practitioner,' 1896, stated that "scurvy is a disease due to want of proper ventilation and want of proper blood nourishment; in fact, scurvy begins with anæmia, and its great antidote is fresh blood." He consequently did not consider that fresh vegetables were of such

* Towards the expenses of this research a grant was received from the Royal Society.

prominent importance in the warding off of scurvy as the general teaching up to the present has led us to believe. These remarks of his were drawn from practical experience in the Arctic regions, as will be later mentioned.

It is to the sojourners in the Arctic and Antarctic climes that scurvy nowadays is of such overwhelming importance, although in some lands nearer home it is still rife, and occasional cases even now occur in our marine services. Credit, as has been already stated, has been given to the use of lime juice in the Royal Navy and the Merchant Service for the great reduction of scurvy on board ship, but, as this research will presently show, this conclusion is probably without justification.

In the Nares Polar Expedition the crews of both the "Alert" and the "Discovery" suffered greatly from this affliction, although lime juice was taken daily by all hands when on board. When on the sledging expeditions, in consequence of the necessities of such a condition of travel, only a small quantity of lime juice was carried, yet an outbreak of scurvy occurred, not only amongst the sledging parties, but also amongst the men that remained on board ship and continued to take the prescribed allowance of lime juice daily.

On the other hand Mr. Leigh Smith's party, with its medical officer, Dr. Neale, after the loss of their ship the "Eira," spent nine months, including a winter, upon Franz Josef Land, in the severest, and, necessarily, the most unsanitary, conditions imaginable. They had no lime juice whatever; however, they almost entirely lived upon freshly killed meat and frozen blood, and no case of scurvy occurred amongst them.

On comparing these two examples we see that in the case of a body of picked men, well housed, well cared for, and with all possible means for procuring health adopted by those in command, taking the prescribed quantity of lime juice daily (except in the case of the sledge crews when absent from the ship for a few weeks) but who lived almost entirely on preserved meat, we have universal scurvy; while, on the other hand, we have a party who had not been selected on account of physical fitness, who were cast upon the desolate shores of Franz Josef Land, and with only the bare necessaries of existence, passed through nine months of their life there under conditions of considerable privation and hardship, and in circumstances which would hardly meet with the approval of any sanitary inspector. They had no lime juice, but lived on freshly killed bear and walrus meat, and no symptoms of scurvy appeared amongst them.

No less striking results were obtained by Dr. Nansen and Lieutenant Johansen. These two individuals, after they left the "Fram," had to spend nine months, including the winter of 1895 and 1896, on Frederick Jackson Island. They were forced to live in a rudely constructed hut,

without any changes of clothing, and no possibilities of washing, that their sanitary conditions were not only of the roughest descriptio but the most unsuitable possible for health, and during the whole this time they had no fresh vegetables whatsoever, and not even an lime juice. The only food they were able to use was fresh walrus bear's meat, which had been preserved simply by the cold. Durin this entire period they ate no salted or tinned meats, and we can pr sume that the bear and walrus meat, as it would freeze almost imm diately on being killed, would remain perfectly fresh.

In consequence apparently of the purely fresh meat, and in spite the most unsanitary conditions, Nansen and Johansen passed th whole winter almost constantly in the dark, without exercise, and y showed no symptoms of scurvy whatsoever. In fact, the results wit these two individuals show that Neale's view, who had laid speci stress on the want of proper ventilation as one of the causes of scurv was not realised, for the ventilation of their hut must have bee extremely bad, as they describe the soot from their blubber lamps bein deposited everywhere.

Before commenting on this a few more examples in reference to th important subject may well be given.

One of us (F. G. J.), when living amongst the Samoyads on Waiga Island, and the Bolshaia Zemelskija Tundra in 1893 and 189 observed some striking facts as to the cause of scurvy.

Amongst those of the Samoyads who invariably winter upc Waigatz, who never take vegetables nor know of lime juice, scurvy unknown. They, however, live entirely upon fresh reindeer mea On the other hand amongst those Samoyads, who in the autum migrate south with the Russian peasant traders from the neighbou hood of Yugor Straits, and live in common with them in the distric adjoining the large rivers in North-East Russia upon salted fish—t chief winter food there until the following May—scurvy is prevalen That this fish is invariably tainted can be testified to from person experience.

In 1893, when at Kharborova, a Samoyad settlement on the Yug Straits, a remarkable case pointing to the cause of scurvy came und his notice. Six Russian priests, whose religion forbade them to e reindeer or other such meats, but allowed salted fish, were left in a h by Siberiakoff, the wealthy mine-owner, to pass the winter, a year two prior to F. G. J.'s. visit. A small Russian peasant boy—whom conversed with—was left to wait upon them. The priests lived almo exclusively on tea, bread, and salted fish; the boy lived upon simil food, except that instead of the salted fish he ate fresh reindeer mea None of them had any vegetables. In the following May, when t Samoyads and peasant traders returned, they found that all the s priests had died of scurvy, whereas the little boy, who had lived upc

fresh meat and had not eaten salted fish, was alive and well, and had buried all his late masters in the snow, he being the only living being in Kharborova in the spring.

In the experiences collected by one of us—F. G. J., during his late expedition to Franz Josef Land in 1894, 1895, 1896, and 1897—we have two parties to consider: that of the crew of the "Windward," who spent the winter of 1894 and 1895 there, no individual of which ever failed to take his prescribed ounce of lime juice daily, and yet scurvy broke out, causing at least one death; and on the other hand the land party on shore who took no lime juice, except two or three of them, who used it as a refreshing drink during the first few months, after which none was used. During the three years that they passed in Franz Josef Land none of them suffered from any symptoms of scurvy. The difference between these parties was principally, if not entirely, due to the meat. The "Windward" party used largely tinned and salted meat; while, on the other hand, the land party principally lived on bear's meat, and when tinned meat was employed, it passed a severe scrutiny in order that as far as possible it might be not even tainted.

From these and other facts it would appear that neither lime juice nor fresh vegetables either prevent scurvy or cure it, and it is not the absence of this which is the cause of the disease, but that scurvy is a disease produced through the eating of *tainted food.*

The view that scurvy is essentially due to poisoning by the ptomaines of tainted animal food was first propounded by Professor Torup, of Christiania, and it would appear from the foregoing evidence that such is the case, for in all the cases above mentioned where any scurvy occurred the men had lived on tinned meats or salted foods.

Confirming this view, Dr. George M. Robertson, of the Perth District Asylum, relates a case of a woman who had become an inmate chiefly owing to her malady having taken the form of eating filth from pigs' troughs. On arrival in the asylum she was found to be suffering from spongy ulcerated gums—in fact, from "land scurvy," and ultimately all the teeth, except the canines, fell out.

In the many instances of scurvy that we have investigated, in no single case have the circumstances rendered inadmissible, or even improbable, the theory that this disease is due to ptomaine poisoning. Before giving to lime juice the credit of having practically swept away scurvy from the naval and marine services, it is necessary to remember that other causes have at the same time been at work to promote health, such as improved sanitation, better quarters for the men, shorter voyages through the enormous increase in the use of steam, and above all *better food*.

The evidence so far shown, in which men have unwillingly experimented on the effects of ptomaine poisoning, proves that scurvy is

produced by the eating of tainted meat, and not by the want of fresh vegetables. In order to confirm or negative this view, it was decided to carry out some experiments in this country. After careful consideration, it was concluded that the most suitable animals for such experiments would be monkeys, since they are mostly nearly allied to man. Monkeys are not naturally carnivorous, and therefore it would be necessary to give the meat mixed with food that would not possess any alleged anti-scorbutic properties; and for this purpose it was decided to feed the monkeys on boiled rice and maize. In order to keep the standard of meat as nearly as possible always the same, a certain brand of tinned Australian beef was employed.

Daily the rice was well boiled, and, after becoming thoroughly softened, 50 grammes of meat was added to each portion of rice for the various monkeys. It was then well stirred and gently heated; by this means the meat got well mixed with the rice, and, although the monkeys might reject some of the larger lumps of meat, a considerable portion of it was eaten. At the same time any soluble ptomaines would be absorbed by the rice, and thus eaten by the monkeys. To this mixture daily was added a certain amount of maize. The results can be best described by dividing the experiments into three groups.

First Group.—The monkeys in this group were given daily, together with their boiled rice, 50 grammes of meat from a *freshly* opened tin, together with maize.

Second Group.—The monkeys in this group were given the same quantities of meat as in the previous group, but from tins which had been opened for a few days, and had stood in the laboratory. The meat in these was not what one would call bad, although it had a distinctly sour smell. Rice and maize as before.

Third Group.—The monkeys in this series were given exactly the same diet as was employed in the second group, except that each monkey received daily either an apple or a banana.

We have found in these three groups three conditions, so far as diet is concerned, which ought to yield definite results in reference to the subject of ptomaine poisoning,

In order that the general surroundings of the monkeys should be as nearly as possible the same, and that each should be properly observed, every monkey was kept in a separate cage of similar construction, so that the fæces could be easily examined. The cages were kept in a room warmed by hot-water pipes, so that they were under as nearly as possible similar conditions as regards light and heat. The excreta of the monkeys were examined daily, and the general appearance of the animals noted, more especially as regards the condition of the gums. Every few days they were weighed.

We can now proceed to describe the results of the experiments. In

order to study the results of each group more easily they are put together in a tabular form.

First Group.—The monkeys fed on boiled rice with 50 grammes of fresh meat and maize daily.

Six monkeys in this group (Table I) were kept under observation.

Table I.—Group 1. Monkeys fed on Boiled Rice, with 50 grammes of Fresh Meat and Maize daily.

No.	Duration of observation.	Weight in kilos. Original.	Final.	Loss.	Diarrhœa commenced.	Blood and mucus in stools.	Gums spongy.
	days.				days.	days.	days.
1	70	2·000	1·450	0·550	62	0	0
2	31	2·250	1·700	0·550	23	0	0
3	28	2·200	1·850	0·350	3	0	0
4	45	2·650	1·950	0·700	7	0	0
5	73	2·750	1·650	1·100	13	0	0
9	30	1·250	1·050	0·200	5	0	0

In the case of No. 5, which lost no less than 1·100 kilos. in seventy-three days, this loss of weight principally occurred during the last seven days, for up to that period this monkey had only lost 300 grammes. During the last seven days, however, severe diarrhœa set in, followed by rapid wasting. The same holds good as regards the other cases. So that although the monkeys on this diet lost weight, possibly from the food not being sufficiently nutritious, the principal loss of weight was apparently due to the diarrhœa.

When we come to consider the diarrhœa, we see that all six monkeys developed this condition sooner or later. As a rule it commenced by being very intermittent, and becoming more severe towards the end, when it speedily proved fatal by the general loss of strength, &c., which it occasioned.

The diarrhœa in monkey No. 1 did not commence until it had been sixty-two days on the diet, when it proved fatal after seven days. In the case of monkey No. 5, on the other hand, it began after thirteen days, but was only slight, and at intervals of three or four days, after which it became very severe; during the last seven days there was rapid wasting, and death occurred on the seventy-third day.

In the case of monkey No. 3, the diarrhœa commenced, however, on the third day, when it was very severe, and afterwards, although the diarrhœa was not so acute and occurred only at intervals, the animal became very feeble, and on the twenty-eighth day was killed by chloroform. At the autopsy no cause for the feeble condition could be

found, although the large intestine was somewhat congested in this case.

In spite of the appearance of this diarrhœa, none of the monkeys in this group showed any signs of either blood or mucus in the motions, the liquid stools being merely of a pale yellowish colour. And in all these cases the gums, although frequently examined, showed no sponginess nor signs of bleeding. The monkeys of this group, as they became emaciated, sat hunched up in their cages, the most usual attitude being with their heads between their knees, as if they were trying to keep themselves warm, although the room was, as already stated, heated by hot-water pipes. They also showed signs of being out of condition by the general roughened condition of their coats.

At the autopsy all these monkeys exhibited more or less marked emaciation, but with the exception of No. 2, which died from pneumonia, in no case was any direct cause of death discoverable. In the bowels were found liquid, light-coloured contents, and only in the case of No. 3 were there any signs of congestion to be noted in the large intestine.

Table II.—Group 2. Monkeys fed on Boiled Rice, with 50 grammes of Tainted Meat and Maize daily.

No.	Duration of observation.	Weight in kilos.			Diarrhœa commenced.	Blood and mucus in stools.	Gums spongy and bleeding.
		Original.	Final.	Loss.			
	days.				days.	days.	days.
6	18	2·000	1·400	0·600	5	0	0
7	14	1·350	1·050	0·300	5	8	18
8	55	1·500	0·950	0·550	17	23	27
10	65	1·600	1·050	0·500	7	28	28
11	54	1·650	1·250	0·400	4	26	27
12	11	2·425	0·150	0·275	6	9	0
13	80	2·050	1·900	0·150	0	0	0
20	62	2·450	1·400	1·050	40	40	40

In the above table (II) the results of eight observations under these conditions are recorded. The monkeys of this group lived from eleven to eighty days, although in the cases of Nos. 8, 10, and 20 we cannot call this the limit of their life, as they were killed in order to examine the influence scurvy, thus artificially produced, would have on their blood.

We see in this Table II that we have the same loss of weight in these monkeys as we had in the six monkeys fed on the fresh meat. In this group the diarrhœa commenced earlier than in those pre-

viously described. In only one case was it delayed forty days; in the other cases it commenced between the fourth and seventh day, except in monkey No. 8, in which it did not commence until the seventeenth day. In these, as in the previous monkeys, diarrhœa, although always occurring, was somewhat intermittent.

Out of those eight monkeys, in no less than six was it seen that the motions were not of the simple diarrhœaic character of the former group, but contained blood and mucus. In monkeys Nos. 7 and 12 the blood and mucus appeared on the eighth and ninth day respectively, while in the other cases it was more delayed, the diarrhœa having continued for some time previous to its appearance. In some of the cases the motions just before death consisted principally of blood and mucus.

When we turn to the appearance of the gums, we find that in five out of the eight monkeys included in this group they showed sponginess, and in some cases even small ulcers forming. The sponginess of the gums was most marked around the incisors and bicuspids, and, as a rule, did not occur around the molars at all.

The monkeys belonging to this group sat in the same cramped position, with roughened coats, as already described in the previous group. They showed a more marked disinclination to move, or to take interest in objects around them; but in no cases did they show any signs of definite tenderness of their limbs when handled. Only in one case (No. 7) was there any indication of bruising. In this monkey a few days before death two bruises developed on his left knee, about ½ cm. in diameter, of a dirty red-brown colour, and also sores showed on the sole of the right foot and at the root of the tail.

In all these cases, as in previous monkeys, an autopsy was carried out. In no single instance was there found any signs of hæmorrhage or hæmorrhages into the pleura, pericardium, or peritoneum. The gums in Nos. 8 and 10 were not only spongy, but had a tendency to the formation of ulcers at the root of the incisors.

The stomach and small intestine showed little or no change, while, on the other hand, the large intestine was, in the majority of cases (except Nos. 6 and 13), markedly congested, the congestion being, as a rule, most noticeable at the sigmoid flexure and cæcum. The contents of the small intestine were light yellow, while the large intestine contained more or less bloody mucus.

The only thing else abnormal to be noticed was that in No. 7 the liver was enlarged and fatty, showing markedly the line of the ribs.

We now come to consider the Third Group (Table III). In this the monkeys were fed on boiled rice with 50 grammes of tainted meat and maize daily, but each monkey received, in addition, an apple or a banana.

Table III.—Group 3. Monkeys fed on Boiled Rice, with 50 grammes of Tainted Meat and Maize daily. Each Monkey received a Banana or Apple, these being given on alternate days.

No.	Duration of observation.	Weight in kilos.			Diarrhœa commenced.	Blood and mucus in stools.	Gums spongy and bleeding.
		Original.	Final.	Loss.			
	days.				days.	days.	days.
14	22	1·600	1·350	0·250	16	18	0
15	180	2·200	1·300	0·900	150	0	0
15	13	1·750	1·500	0·250	8	11	11
17	31	2·476	1·500	0·975	9	20	present*
21	123	2·000	1·650	0·350	90	90	0

In this group the animals, therefore, in addition to the rice and maize and tainted meat, received fresh vegetables daily, and may be considered to have been well fed. Five monkeys only were used, and, in spite of the extra food, they all lost weight. In fact, Nos. 15 and 17 lost no less than 900 and 975 grammes of weight in 180 and 31 days respectively. The others, however, did not lose so much weight. In three of the monkeys, Nos. 14, 16, and 17, diarrhœa commenced from eight to sixteen days after the monkeys had been on this diet. But in No. 15 diarrhœa did not commence until the 150th day of observation, and in No. 21 not until the 90th day. In four of the cases blood and mucus appeared in the motions in from eight to eighteen days. When it first occurred in No. 21 was unfortunately not noticed. The other monkey, No. 15, did not show any signs of it. In two of the monkeys, Nos. 16 and 17, spongy gums occurred; in the former, on the eleventh day, while in the latter it was only noted at the autopsy. The other three monkeys showed no signs of bleeding gums.

The autopsy of those monkeys which showed scurvy exhibited, as in those of the second group, marked congestion of the large intestine, with bloody mucus in the contents.

In all the cases there was marked emaciation but no hæmorrhage, either into the pleura, pericardium, or peritoneum.

After this general description of the results obtained in the three groups of experiments, and before discussing their significance, we can consider the changes produced in the blood of animals suffering from the results of scurvy—scurvy being defined by Bristow as "a peculiar form of anæmia arising from a deficiency of vegetable diet, and attended with a tendency to the occurrence of hæmorrhages, profound impairment of nutrition, and great mental and bodily prostration."

* Only noted at the autopsy.

Blood in the Scurvy of Monkeys.—In order to get monkeys with as well-developed symptoms of scurvy as possible, the animals were kept until not only were they passing bloody mucus by the bowels, but the gums were spongy and easily bled. Unfortunately, the blood of only two monkeys could be examined, as the others died too speedily. The monkeys Nos. 8 and 10 in the second group, however, both showed very well marked symptoms of scurvy, as found in monkeys—diarrhœa, wasting, the motions containing blood and mucus, and the gums spongy and easily bleeding.

Table IV.—Comparing Analysis of Blood of a Normal Monkey with that of two suffering from Scurvy.

	Normal.	Scurvy.	
Weight in kilos	2·000	0·950	1·050
Number of corpuscles	4,730,000	4,220,000	4,500,000
" " leucocytes	8125	40,000	—
Hæmoglobin	75	48	45
Specific gravity	1046	1035	1034
Water	83·37	85·18	84·99
Solids	16·63	14·82	15·01
Proteids	18·27	12·37	15·69
Fibrine	0·52	—	0·76
Time of coagulation	3 minutes	2 minutes	1 minute
N.	2·72	2·31	—
Ash	0·75	0·79	—

In the above Table IV the results of the analyses of the blood in the case of these two monkeys are compared with that of a normal monkey, so that we can more readily see the changes produced.

The blood to be examined was collected from the carotid artery while the animal was kept under the influence of ether, and when sufficient blood had been collected for the various analyses, the animal was killed by an overdose of anæsthetic before it returned to consciousness.

The Number of Red Blood Corpuscles.—These were estimated by the Thoma-Zeiss' Counter, and in each of these cases the average of two blood counts is given, the result being the number of red blood corpuscles contained in a cubic millimetre of blood.

It is seen that while in the normal monkeys the number of red blood corpuscles is 4,730,000 per cubic millimetre, in the two monkeys suffering from scurvy it is respectively 4,220,000 and 4,500,000.

Other observers have drawn attention to the diminution in the number of red corpuscles in cases of scurvy as quoted by Philip Wales in Ashurst's 'International Encyclopædia of Surgery.'

From these results one may conclude that there is a slight reduction in the number of red blood corpuscles in monkeys fed on tainted meat, although this reduction is nothing like the reduction one finds in the human subject in many of the more severe forms of anæmia.

The Leucocytes.—The white blood corpuscles were counted in the same way as the red blood corpuscles. This was carried out on one of the monkeys affected wtih scurvy. It is seen in the case of the normal monkey that there were 8125 leucocytes per cubic millimetre, while in the case of the monkey suffering from scurvy there were no less than 40,000.

Laboulbène notes the occurrence of an unusual number of white globules in scurvy. We certainly can conclude that there is a very marked leucocytosis produced by the diet of tainted meat.

The Hæmoglobin.—The quantity of colouring matter in the blood was estimated by Fleischl's hæmometer. In each case precautions were taken to use the same illumination power, and at least two calculations were made, the average being taken.

It is seen in the above table (IV) that the hæmoglobin present represent 75, while in both the monkeys suffering from scurvy it is very considerably reduced, being 48 and 45 respectively. When we compare this with the small reduction in the number of red blood corpuscles, we see that in the monkeys fed on tainted meat there is produced a very marked hæmoglobinæmia, while at the same time there is probably oligocythæmia.

In fact this condition corresponds with that in the human being in cases of chlorosis, as against those forms of pernicious anæmia or secondary anæmia, where either the red blood corpuscles are reduced out of proportion to the hæmoglobin, or the hæmoglobin and corpuscles are reduced in equal proportions.

The Specific Gravity of the Blood.—The estimation of the specific gravity of the blood in these monkeys was carried out by means of the picnometer. In the normal monkey we have a specific gravity of 1046, while in the case of the two monkeys suffering from scurvy we have a specific gravity of 1035 and 1034; so that we can conclude that there is a slight decrease in the specific gravity produced by the diet of tainted meat.

The Water and Solids of the Blood.—A given quantity of blood was collected in a platinum crucible and dried to constant weight at 70° C. It is seen that in the normal blood there was 83·37 per cent. of water and 16·63 per cent. solids, while in the case of the monkeys suffering from scurvy the percentage of water was 85·18, with only 14·12 per cent. of solids, in monkey No. 8, while 84·99 per cent. of water, with 15·01 per cent. of solids, occurred in No. 10. It would appear that all observers who have noted the blood in scurvy speak of the marked diminution in the total solids which occurs in this disease. So that we

have with the decrease in the specific gravity of the blood a diminution in the total solids, as one naturally would expect.

The Proteids of the Blood.—The total quantity of proteids in the blood was estimated by precipitating a given quantity of blood in 15 volumes of absolute alcohol. After allowing it to stand some days, with frequent stirring, the precipitate was collected on a weighed filter paper, and dried, &c., in the usual manner. By this method it was found that the normal blood contained 18·27 per cent. of proteids, while in the case of the two monkeys fed on tainted meat the quantity of proteids was only 12·37 and 15·69, so that there is a very marked decrease in the quantity of proteids in the blood.

The Quantity of Fibrine.—This was estimated by the method of Hoppe-Seyler in one case. It is seen that in the normal monkey there is 0·52 per cent. of fibrine in the blood, while in the case of the monkey suffering from scurvy the fibrine was no less than 0·76, so that we see there was a very marked increase in the quantity of fibrine.

Chalvet, in his analysis of a case of scurvy, comparing it with healthy blood in the normal individual, found the fibrine 0·216, while in the case of scurvy it was 0·434.

Busk, in three well-marked cases of scurvy which occurred in the "Dreadnought" hospital ship, found the fibrine in excess of the normal amount, the least being 0·45 and the greatest 0·65 per cent.

The Time of Coagulation.—The time that it took for the blood to coagulate was estimated by Professor Wright's tubes, and it is seen that in the normal monkeys this is three minutes, while in the case of the monkeys suffering from scurvy it was found to be one and two minutes respectively.

The increase in the quantity of fibrine (hyperinosis) with the shortening of the time of coagulation is what one commonly finds in hydræmia in the human subject, and may therefore in all probability be put down to the same cause.

The Quantity of Nitrogen.—The total quantity of nitrogen in the blood was estimated by the method of Kjeldahl, and the average of two analyses is given as before. In the normal monkey it is seen that the total nitrogen was 2·72, while in the case of the monkey suffering from scurvy, in which it was analysed, it was 2·31 per cent. There is, therefore, a small decrease in the quantity of nitrogen, this decrease corresponding to the decrease in the quantity of proteids, and as this was only analysed in one monkey, it is as well perhaps not to discuss any theories as to its significance.

The Ash of the Blood.—The estimation of the ash was carried out in the ordinary way, but only in the case of one monkey suffering from the effects of tainted meat. It is seen that in the case of the normal monkey the ash was 0·75, while in the case of the monkey fed with tainted meat it was 0·79 per cent.

The average amount in Becquerel's, Rodies's, and Busk's cases of scurvy in the human subject was 0·81, the smallest being 0·55 and the largest 1·15 per cent.

Garrod, in the analysis of the blood in one case of scurvy, found a deficiency of the potassium salts, upon which he formed his well-known theory that scurvy was due to their want.

We therefore see, in reference to this monkey, that the ash does not tend to be decreased with the low specific gravity and diminution of the total solids. It is apparent that the diminution in the total solids is principally due to a lessening in the quantity of proteids.

Conclusions.

The descriptions brought forward in the first part, of several cases of scurvy which occurred in the Arctic regions when the individuals were under the influence of preserved or salted meats, in spite of their taking at the same time either vegetables or lime juice, of the Nares Polar Expedition, in which scurvy occurred, as well as the very striking case of the six priests already mentioned, can be compared on the other hand with conditions of the greatest hardship and privation in the Leigh Smith Expedition, Nansen and Johansen, and the Frederick Jackson land party, as well as in the instance given of the Samoyads who winter on Waigatz and who live on fresh meat; in all of which cases, in spite of the entire absence of vegetables or even lime juice, no scurvy occurred.

If we look at this evidence alone we could almost say we have conclusive experimental evidence that the eating of salted or improperly preserved meat, or tainted meat in any form, can produce scurvy, even when lime juice or vegetables are being taken at the same time.

We have also the support of the fact that bad ventilation, believed by Dr. Neale to be one of the causes of scurvy, was not the cause of scurvy with Dr. Nansen, living on fresh meat and blood—probably owing to the fact that he introduced no ptomaine, and therefore no scurvy occurred.

We now come to consider the experiments on monkeys, and how much or otherwise these experiments confirm the results already given in man. It is necessary to consider what are the symptoms of scurvy. In the present paper it is impossible to go through all the symptoms of scurvy described by the various observers, since different epidemics have shown more markedly various symptoms. We, however, can compare the symptoms in our monkeys with those generally described as accompanying scurvy. The pallor and yellowish colour of the face, which is described as distinctive in scurvy, is of course impossible to be observed in monkeys, although in those monkeys which we consider to suffer from scurvy there was generally a good deal of blue-

ness about the lips and gums. There was a very marked disinclination to bodily movement and a general tendency to mental prostration, for the monkeys took little interest in the things surrounding them, in those cases which showed what we might consider the more definite symptoms of scurvy, such as the bloody mucus and bleeding gums.

At the same time in none of these monkeys did we find any definite tenderness of the limbs, no swelling of the legs, or any purpura. Only in one case do we get the formation of bruise-like sores in an animal which apparently was suffering from scurvy.

In the monkeys included in the first group, which were fed on fresh meat as well as maize and rice, the only symptom we note beyond the wasting is diarrhœa, and none of these monkeys showed anything like the muscular feebleness or general ill-health which was noted in the scorbutic monkeys.

When we compare the second group, Table II, in which the monkeys received the same diet, except that the meat instead of being fresh was tainted, we find a very different state of affairs. These monkeys showed a very much greater prostration, and although it is difficult to judge by the eye they certainly seemed paler and generally out of condition. No less than six out of the eight monkeys thus fed passed blood and mucus in their motions.

The question whether blood and mucus in the motions is to be regarded as one of of the symptoms of scurvy, can be easily answered by the fact that, first, Bristow states that "patients suffer from looseness of the bowels, the motions frequently being highly offensive and containing blood." It is also stated by other observers, such as Hilton-Fagge and Osler, as well as in an able article on scurvy in Ashurst's 'International Encyclopædia of Surgery,' by Philip Wales, that the occurrence of hæmorrhages in the mucous membrane of the stomach and bowels is of frequent occurrence.

None of our monkeys vomited, so that whether they suffered from hæmorrhages in the stomach cannot be noted. In the *post mortem* there was no evidence of any such thing.

If this bloody diarrhœa is an evidence of scurvy, we find that no less than six out of the eight monkeys which were given the tainted meat showed this symptom.

We now compare the third group, Table III, in which the monkeys were given fresh fruit, apples or bananas, every day. One can say that the monkeys in this case were well fed. Five monkeys were observed, and out of these five monkeys no less than four developed the symptoms of bloody mucus in their stools, so that in spite of good feeding this symptom of scurvy developed in four out of the five monkeys on full diet.

We come to the next symptom, undoubtedly the most definite sign of scurvy that occurs in man—in fact, it is about the only condition

which is universal in scurvy, and is always found except in those cases in which the teeth are absent—that is, the spongy condition of the gums. When we consider the first group of monkeys which were merely fed on rice, maize, and fresh meat, out of the six monkeys not one showed any appearance of spongy gums, so that we can conclude that in these monkeys none of them showed any scurvy whatever.

We now come to the eight monkeys fed on exactly the same diet, in which the meat was tainted instead of fresh, and here no less than five of the eight monkeys showed sponginess of the gums, and some, not only the sponginess but the gums even ulcerated, so that five out of the eight monkeys showed this sign, which is considered by all who describe scurvy as the most significant.

We now come to those monkeys which were well fed, getting the fresh vegetables every day. Four of these monkeys showed bloody mucus in the stools, and two of them spongy and bleeding gums.

We have further to consider the condition of the blood in the monkeys fed on tainted meat. Scurvy is considered by all authorities to be a peculiar form of anæmia. In two monkeys we had the opportunity of analysing the blood, and it is seen by the analyses that we have marked changes in the blood, a very great diminution in the quantity of hæmoglobin, with a slight diminution in the number of red blood corpuscles—in fact, a condition corresponding to chlorosis, and that is accompanied by leucocytosis.

The specific gravity of the blood is reduced, and this is due to the reduction in the quantity of proteids and not to a marked reduction in the quantity of salines in the blood. The fibrine of the blood is increased, together with an increase in the coagulability of the blood.

When we consider what has been found in the blood of man suffering from scurvy, we find it is universally accepted that there is this condition of anæmia with low specific gravity, the blood being distinctly watery and with a marked excess in the quantity of fibrine. So the condition of the blood of the two monkeys which we have analysed corresponds with that found in the majority of analyses of scorbutic blood.

Considering therefore the occurrence, in the monkeys fed on tainted meat, of bloody mucus in the stools, spongy gums, and characteristic anæmia, we assume that, although the symptom of hæmorrhages into the tissues was not observed, we may fairly conclude that they were really scorbutic.

This conclusion is further justified when we consider, on looking over the description of scurvy given by authors, that all the symptoms except the spongy gums are very often absent in different epidemics.

The fact that the five monkeys fed on tainted meat, in which fresh vegetables were given, showed in a smaller proportion the symptoms of scurvy than the monkeys in the second group, can be sufficiently

explained by the fact that when the monkeys received a banana or an apple every day, they would be less likely to eat as much of the meat as they would otherwise do, and would thus daily receive a smaller dose of ptomaine.

In spite of this fact, in no less than four cases out of the five did we get bloody motions, and in two of the cases spongy gums. In these cases tainted meat alone seems to have produced scorbutic symptoms, although the animals in this group took longer to develop the symptoms, and seemed not to suffer in such a severe form.

Looking at the results of our experiments on monkeys, as a whole, we venture to think that they afford important confirmation of the conclusion derived from Arctic experience, as referred to in the early part of this paper, that the absence or presence of fresh vegetables or lime juice is not alone sufficient for the prevention or the cure of scurvy, and that we must regard the condition of the food in general, and especially the state of preservation of the meat, as the essential factor in the etiology of the disease.

We have to express our thanks to Dr. Francis Goodbody for his untiring assistance in the numerous observations that had to be made during the eighteen months the research was continued.

"The Theory of the Double Gamma Function." By E. W. Barnes, B.A., Fellow of Trinity College, Cambridge. Communicated by Professor A. R. Forsyth, Sc.D., F.R.S. Received February 26,—Read March 15, 1900.

(Abstract.)

The memoir deals with a function which is substantially one-quarter of the σ function of Weierstrass, just as the ordinary (simple) gamma function is substantially one-half of the infinite sine product. The analogy between the two functions determines the nomenclature.

In any development of the simple gamma function from the function-theory point of view, it is necessary to use Euler's theorem

$$\underset{n=\infty}{\mathrm{L}t}\left[1+\tfrac{1}{2}+\ldots\ldots+\frac{1}{n}-\log n\right]=\gamma$$

to obtain the product $\Gamma_1(z/\omega)$ given by

$$\Gamma_1^{-1}(z/\omega) = \omega^{-\frac{z}{\omega}} e^{\frac{\gamma z}{\omega}} \,.\, z \,.\, \prod_{n=1}^{\infty}{}' \left[\left(1+\frac{z}{n\omega}\right)e^{-\frac{z}{n\omega}}\right]$$

as a solution of the difference equation $f(z+\omega) = zf(z)$.

Similarly for the elementary theory of the double gamma function

investigated in Part II of the memoir, two forms, $\gamma_{21}(\omega_1, \omega_2)$ and $\gamma_{22}(\omega_1, \omega_2)$, are considered which arise substantially as finite terms in the approximations for

$$\sum_{m_1=0}^{n} \sum_{m_2=0}^{n}{}' \frac{1}{\Omega^2} \quad \text{and} \quad \sum_{m_1=0}^{n} \sum_{m_2=0}^{n}{}' \frac{1}{\Omega},$$

where $\Omega = m_1\omega_1 + m_2\omega_2$, when n is large. These approximations are shown to involve logarithms of ω_1, ω_2, and $(\omega_1 + \omega_2)$; and the relative distribution of the points in the Argand diagram representing these quantities causes the introduction of two numbers m and m' of fundamental importance in the theory. The double gamma function $\Gamma_2(z/\omega_1, \omega_2)$ given by

$$\Gamma_2^{-1}(z)\,\omega_1, \omega_2) = e^{\frac{z^2}{2}\gamma_{21}(\omega_1, \omega_2) + z\gamma_{21}(\omega_1, \omega_2)} \cdot z \cdot \prod_{m_1=0}^{\infty} \prod_{m_2=0}^{\infty}{}' \left[\left(1 + \frac{z}{\Omega}\right) e^{-\frac{z}{\Omega} + \frac{z^2}{2\Omega^2}}\right]$$

is shown to satisfy the two difference relations

$$\frac{\Gamma_2^{-1}(z + \omega_1)}{\Gamma_2^{-1}(z)} = \frac{\Gamma_1(z/\omega_2)}{\rho_1(\omega_2)} e^{-2m\pi i S_1'(z/\omega_2)},$$

$$\frac{\Gamma_2^{-1}(z + \omega_2)}{\Gamma_2^{-1}(z)} = \frac{\Gamma_1(z/\omega_1)}{\rho_1(\omega_1)} e^{-2m'\pi i S_1'(z/\omega_1)}.$$

The functions $\gamma_{21}(\omega_1, \omega_2)$ and $\gamma_{22}(\omega_1, \omega_2)$ are called the first and second double gamma modular forms respectively.

The double gamma function can be expressed in two ways as an infinite product of simple gamma functions; it can be connected with an unsymmetrical function $G(z/\tau)$ first considered by Alexeiewsky; and in terms of it, Weierstrass' elliptic functions can be expressed. By means of the values of the numbers m and m' the well-known relation

$$\eta_1\omega_2 - \eta_2\omega_1 = \pm \tfrac{1}{2}\pi i$$

can be obtained, as well as the fundamental formulæ of the σ function.

Fundamental in the theory of double gamma functions is the double Riemann ζ function, $\zeta_2(s, a/\omega_1, \omega_2)$, which is considered in Part III, and is the simplest solution of the difference equation

$$f(a + \omega_1 + \omega_2) - f(a + \omega_1) - f(a + \omega_2) + f(a) = \frac{1}{a^s},$$

where s, a, ω, ω_1, and ω_2 have any complex values such that ω_2/ω_1 is not real and negative, and a^{-s} has its principal value with respect to the axis of $-(\omega_1 + \omega_2)$. This function is expressible as a contour-integral by means of the relation

$$\zeta_2(s, a) = \frac{i\Gamma(1-s)}{2\pi} e^{2M s\pi i} \int \frac{e^{-az}(-z)^{s-1} dz}{(1-e^{-\omega_1 z})(1-e^{-\omega_2 z})},$$

and the determination of the axis of the contour and the number M depends upon the distribution of the points ω_1, ω_2, -1, and $(\omega_1+\omega_2)$.

By means of this function we obtain asymptotic approximations for summations of the type

$$\sum_{m_1=0}^{pn} \sum_{m_2=0}^{qn} \frac{1}{(a+m_1\omega_1+m_2\omega_2)^s}$$

when n is a very large number and s has any complex value. We can also obtain an asymptotic approximation for the product

$$\prod_{m_1=0}^{pn} \prod_{m_2=0}^{qn} (a+m_1\omega_1+m_2\omega_2).$$

Since Stirling's theorem gives the asymptotic evaluation of $n\,!$, we obtain, on putting $a = 0$, an extension of Stirling's theorem to two parometers. We find, as the absolute term, the double Stirling function $\rho_2(\omega_1, \omega_2)$, which is the analogue of the simple Stirling form $\rho_1(\omega) = \sqrt{2\pi/\omega}$. All the double asymptotic expansions involve as their coefficients double Bernoullian functions and numbers. The double Bernoullian function ${}_2S_n(a/\omega_1, \omega_2)$ is an algebraic polynomial which satisfies the two difference equations

$$f(a+\omega_1) - f(a) = S_n(a/\omega_2) + \frac{S'_{n+1}(0/\omega_2)}{n+1},$$

$$f(a+\omega_2) - f(a) = S_n(a/\omega_1) + \frac{S'_{n+1}(0/\omega_1)}{n+1},$$

and possesses properties exactly analogous to the corresponding simple forms. The theory of this function forms Part I of the memoir. From the contour-integral expression for the double Riemann ζ function it is possible to obtain similar expressions for the logarithm of the double gamma function and its derivatives, for the first and second double gamma modular forms, and for the logarithm of the double Stirling function. Under certain restrictions these contour-integrals can be transformed into line-integrals.

The double gamma function admits of transformation and multiplication theories developed in Part IV. By means of the latter theory we may express the double Stirling form as a product of double gamma functions of arguments $\frac{1}{2}\omega_1$, $\frac{1}{2}\omega_2$, and $\frac{1}{2}(\omega_1+\omega_2)$ respectively. There is also a transformation theory for the double gamma modular forms and the double Stirling function.

The extension of Raabe's formula for the simple gamma function leads to certain "integral formulæ." The integral

$$\int_0^z \log \Gamma_1 (z)\, dz$$

can be expressed in terms of double gamma functions of equal parameters, and the two integrals

$$\int_0^{\omega_1} \log \Gamma_2 (z)\, dz, \qquad \int_0^{\omega_2} \log \Gamma_2 (z)\, dz$$

can be substantially expressed in terms of the double Stirling function of ω_1 and ω_2.

It is shown in Part V that it is possible to obtain an asymptotic approximation when $|z|$ is very large, for

$$\log \Gamma_2 (z + a),$$

which is valid over that part of the region at infinity in which there are no zeros of $\Gamma_2^{-1}(z)$. The coefficients in this expansion are double Bernoullian functions of a.

Finally it is proved that the double gamma function does not satisfy any differential equation whose coefficients are not substantially that function or its derivatives.

There exist n-ple gamma functions whose properties can be obtained by an easy generalisation of the previous theory.

March 22, 1900.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Croonian Lecture—" On Immunity, with Special Reference to Cell Life "—was delivered by Professor PAUL EHRLICH, of Frankfort-on-the-Main.

March 29, 1900.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Retinal Currents of the Frog's Eye, excited by Light and excited Electrically." By A. D. WALLER, M.D., F.R.S.

II. "Observations on the Electromotive Phenomena of Non-medullated Nerve." By Miss S. C. M. SOWTON. Communicated by Dr. WALLER, F.R.S.

III. "Variation." By Professor J. C. EWART, F.R.S.

IV. "Certain Laws of Variation." By Dr. H. M. VERNON. Communicated by Professor LANKESTER, F.R.S.

V. "Data for the Problem of Evolution in Man. IV. Note on the Effect of Fertility depending on Homogamy." By Professor KARL PEARSON, F.R.S.

VI. "Mathematical Contributions to the Theory of Evolution. VII. On the Inheritance of Characters not capable of Exact Quantitative Measurement." (Revised.) By KARL PEARSON, F.R.S.

"Thermal Radiation in Absolute Measure." By J. T. BOTTOMLEY, M.A., D.Sc., F.R.S., and J. C. BEATTIE, D.Sc., F.R.S.E. Received November 28, 1899,—Read February 1, 1900.

The experiments* described in the following paper form a continuation of researches on thermal radiation by one of the present authors, the results of which have been communicated to the Royal Society from time to time since 1884.† The main object of the present experiments was to push forward the inquiry as to the amount, and the relative quality, of the radiation from surfaces of various kinds in high vacuum.

When a body is maintained at a high temperature the total radiation from its surface depends, other things being the same, on the temperature and on the character of the radiating surface. With a given temperature the total radiation, consisting of thermal, luminous, and actinic rays, seems to depend on the nature and on the ultimate texture of the radiating surface; and the proportion in which vibrations of longer and shorter period are present seems to be governed by the

* The experimental results of the paper were obtained two years ago. Various circumstances have prevented earlier publication; and it was originally intended to carry the investigation further before publishing. Want of opportunity, however, makes this difficult for the present; and we therefore deem it advisable to put our results on record just now, as they stand. The present investigation, as well as the former work referred to in the text above, has been assisted by grants from the Government Grant Fund.

† "On Thermal Radiation in Absolute Measure," J. T. Bottomley, 'Roy. Soc. Proc.' and 'Phil. Trans.,' 1884—1893.

coarseness or fineness of the structure of the surface at which the rays take their origin.

Very little progress has yet been made towards an investigation of the question just referred to; and the results of our experiments are intended to be a contribution in this direction.

In a former paper* by one of us the loss of heat in vacuum from the metallic surface of platinum wires was determined; and Schleiermacher† has compared the loss from bright platinum wires and from platinum wires whose surface was coated thinly with black oxide of copper. Further experiments on this part of the subject seemed highly desirable, and were, therefore, undertaken by us.

The radiating body was a platinum wire. The way in which it was mounted is shown in figs. 1 and 2.‡ The platinum wire, *ab*, is held,

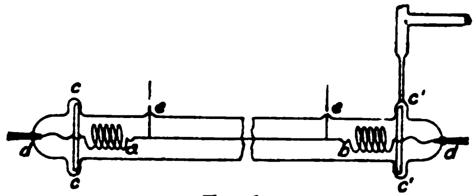


FIG. 1.

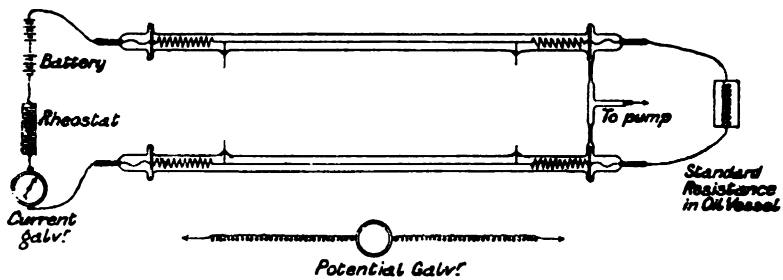


FIG. 2.

stretched between two spiral springs, in a glass tube. The outer ends of the spiral springs terminate in loops; and two pieces of glass rod, which are passed into tubes, *cc*, *c'c'* (see figure), pass through the loops, so that the springs pull on these glass rods. After the rods have been passed into their places, the ends of the tubes *cc*, *c'c'* are closed up, except one, which is used for exhausting. Flexible electrodes are soldered to the loops of the spiral springs, and are silver soldered to

* Bottomley, 'Roy. Soc. Proc.,' 1887.

† Schleiermacher, 'Wiedemann Annalen,' vol. 26, 1885.

‡ The arrangement has been already described, J. T. Bottomley, 'Phil. Trans.,' A, 1887, vol. 178, p. 448

stout multiple platinum terminals; and by means of these, which are fused, with the help of some white enamel, into the glass at *d,d*, the current is passed through the platinum wire. At *e,e*, platinum wires are brought through the sides of the tube, and serve as potential electrodes; and it is to keep the platinum wire *ab* in the middle of the length of the tube, and to avoid pulling unduly on the potential electrodes, that the two spiral springs, one at either end of the tube, are introduced.

Two exactly similar tubes were employed, as shown in fig. 2. They were connected together by a side tube, as shown; and by means of a branch tube, attached to this side tube and connected to a Sprengel pump, the air was withdrawn from both tubes at the same time; and by this arrangement it was provided that the vacuum in the two tubes should be at all times precisely the same.

In one of the tubes the platinum wire was brightly polished and perfectly smooth, just as it came from the maker's hands. The other tube contained a platinum wire cut from the same hank, but with the surface covered with an excessively fine coating of soot. The soot was put on by passing the wire carefully through the upper part of a clear paraffin flame.*

The usual arrangements were made for drying the vacuum of the tube, and of the pump, by means of phosphorus pentoxide; and the vacuum was measured by means of the Gimmingham modification of the McLeod gauge.

The wires were heated, as in the former experiments, by means of an electric current. Fig. 2 shows the electric connections. A battery consisting of a sufficient number of secondary cells was employed; and the current was controlled by means of suitable resistances, including a rheostat. In the experiments here described the platinum wires of the two tubes, the resistances, and the battery were all connected in series, so that the same current passed through all.†

The current in the circuit was measured by means of a Kelvin ampere gauge, and the difference of potentials at the two ends of each

* The texture of the soot depends greatly on the source from which it is obtained and on the way in which it is applied to the wire. Some preliminary experiments have been made with various coatings of soot, and comparisons have been attemped between surfaces finely coated with soot, and surfaces prepared with platinum black and with a fine coating of black oxide of copper chemically applied to the wire (*cf.* J. T. Bottomley, 'Phil. Trans.,' A, 1887, p. 449).

† In another set of experiments the platinum wires were joined in parallel, and, by means of two rheostats, one connected in series with each platinum wire, an attempt was made to regulate the current in each wire so that the temperatures in the two should be the same. This was found very difficult to carry out; but it is intended to renew the attempt, and determine simultaneously the radiation from two wires with different surfaces, in the same vacuum, and at the same temperature.

of the platinum wires was measured by means of a high resistance reflecting galvanometer.

This potential galvanometer had a resistance of about 5000 ohms, and it was possible to insert in the galvanometer circuit an additional resistance of 10,000 ohms.

In order to ascertain the absolute value of the readings of the potential galvanometer, a standard coil of platinoid wire, whose resistance was known very accurately, was joined into the circuit, as shown in fig. 2. This resistance was of considerable length, and it was kept cool by being immersed in a bath of oil.

The following was the order of experimenting. The pressure in the tubes was first reduced as much as possible by means of the Sprengel pump; then a very small current, practically unable to heat any part of the circuit, was sent through the two platinum wires and the standard coil, and the potential difference between the two ends of each was determined. This gave the ratio of the resistance of each of the platinum wires to that of the standard coil, all being cold, and at the same temperature. The current from the battery of storage cells was now suitably increased, and readings were taken in the following order :—The current passing was first read. Then the zero of the potential galvanometer was noted, and the deflection of the potential galvanometer when connected to the two ends of the standard coil was observed. The electrodes of the potential galvanometer were next applied to one of the platinum wires, and the deflection noted; then the deflection due to the second wire was observed. A second reading was taken from the first wire and also from the second wire. Usually these pairs of readings were identical, or nearly so, as no reading was taken until after the strong current had been passing through the circuit for sufficient length of time to allow the temperature of the whole to become perfectly steady. Generally speaking, five minutes or more was allowed for this purpose. Lastly the current was again read, and the zero of the potential galvanometer noted.

The readings detailed above enabled us to calculate the current passing through each wire and the resistance in that wire. The length and cross section of each of the platinum wires (practically identical) were also known. Thus the energy lost by radiation per square centimetre per second, C^2R/JS, could be calculated; C being the current, R the resistance and S the surface of the radiating wire, and J being the dynamical equivalent of heat, all in absolute measure.

The measurement of the electric resistance of the wires also enabled us to calculate the temperatures of the wires by means of the results of a separate determination of the electric resistances, at different temperatures, of the wires themselves.

In a former paper,* the precautions and difficulties connected with

* J. T. Bottomley, 'Phil. Trans.,' A, 1887.

the determination of change of resistances of platinum wires with temperature have been fully discussed. In the present case each platinum wire, after having been used in the radiation experiment, was wrapped round the bulb of an air thermometer* of special construction; the bulb and wire were then packed in asbestos wool, and placed in the laminated copper heating jacket described and figured in the paper just referred to. The jacket was heated by means of one of Fletcher's powerful "solid flame" burners, by means of which it could be kept for any length of time almost absolutely steady, at any temperature below the softening point of glass.

By means of stout copper electrodes the platinum wire was made one of the branches of a Wheatstone balance, and the electric resistance and temperature were simultaneously determined. A considerable number of readings between 15° C. and 350° C. were taken, and from these an empirical formula was constructed, or a curve drawn to represent the relation between temperature and pressure at all intermediate points.

In one respect, the determinations, an account of which is given in the present paper, are not perfectly satisfactory. We have not been able to take account in a proper way of the temperature of the enclosing envelope. In order to be able to see the condition of the wires, and in particular to observe their appearance when they became luminous, glass envelopes were used in these experiments; and owing to the nature of the arrangements and the method of experimenting, it was not found possible to immerse the glass envelopes in a cooling bath. Consequently the glass became more or less heated during the experiments, and the heating was unequal in the cases of the bright wire and the sooted wire. It has already been pointed out† that the proportions in which the radiations of longer period and shorter period are present in the total radiation depends on the radiating surface, other things being the same. In the case of the sooted wire, the quantity of long-period radiation is, in proportion, far in excess of that proceeding from a bright metallic polished surface. Consequently, with the same total electric energy supplied to both wires, the glass tube containing the sooted wire became much hotter than the tube containing the bright wire.

It has also been pointed out‡ that with a substance like glass, conducting badly and somewhat diathermanous, it is impossible to tell how

* J. T. Bottomley, "On a Practical Constant-volume Air Thermometer,' Proc. Roy. Soc. Edin.,' December 19, 1887, and 'Phil. Mag.,' August, 1888. This thermometer has proved perfectly satisfactory; and the separation of the volume gauge and pressure gauge make it extremely convenient for applications of the kind referred to in the text.

† 'Phil. Trans.,' A, 1887, p. 450.

‡ *Loc. cit.*, p. 444.

much heat is returned to the radiating wire from the interior skin of the tube, which no doubt rises to a high temperature during the experiment. To a certain extent, therefore, the results which we have obtained must be considered as not affording results strictly comparable with those formerly obtained in which a metallic envelope cooled with water was used.

The absolute value of the radiation observed ought to be somewhat lower in amount than would have been found had the enclosing envelope been of metal and properly kept cool, and the disturbance from this cause must have been relatively greater in the case of the dull, than in the case of the bright, wire.

Experiments were made with platinum wires from three separate hanks. A pair of wires of equal length was taken in each case. One of these was left with its surface exactly as it was on being taken from the hank; the other was sooted. The two wires were then fixed in the glass tubes. The wires are designated $\overbrace{Pt_1, Pt_2}$, $\overbrace{Pt_3, Pt_4}$, $\overbrace{Pt_5, Pt_6}$. The first of each pair is the bright wire; the second is the sooted wire. The diameters of the wires are as follows:—Pt_1 and Pt_2, 0·0542 cm., Pt_3 and Pt_4, 0·025 cm.; Pt_5 and Pt_6, 0·015 cm.

In Tables I and II specimens are given of the results obtained, in the manner described above, by observation and calculation. The remainder of the results are embodied in the curves appended, which it is hoped will be found self-explanatory. At the head of each table the particulars as to the wires referred to in the table are given.

In the following tables, III, IV, V, the loss of heat per square centimetre of surface for the several pairs of wires, bright and sooted, at various temperatures, is compared; and the ratio between the radiation from the sooted wire and the radiation from the bright wire is calculated. It will be seen that the numbers are in fair agreement. What may be the causes of divergence from exact agreement it is impossible to say at the present stage of the inquiry; but it may be conjectured that part of it at least is due to the difficulty, or impossibility, of keeping the vacuum which surrounds the wires in these experiments unchanged. When the pressure is very low, the accession of the smallest quantity of gas to the surrounding space causes an enormous change in the rate of loss of heat, as has been shown in a previous part of this research; and as the temperature rises it is always found that the vacuum becomes deteriorated, owing to the expulsion of gas from the body of the wire itself. This gas must be removed by a fresh application of the pump, and, in fact, during the experiments the pump must be kept always at work. Thus the vacuum is incessantly changing; and, moreover, as the indications of the McLeod gauge lag very much behind, it is not even possible to know the exact state of the pressure at the instant when it is desired to make an observation as to current

passing and resistance. The consequence is that owing to the constantly altering state of the vacuum an irregularity is introduced in the loss of heat, and the irregularity tells more in the case of small wires than in the case of larger sizes.

In the case of the bright wire, Pt_5, the loss of heat was somewhat abnormal. It is probable that the surface was lacking in polish.

It will be seen that the loss from the sooted platinum wires is about four to five times that from the bright wires at the same temperature. In the paper of 1894, already referred to, the radiation from a very brightly polished and burnished silvered copper globe, and that from the same globe sooted, were determined. The highest temperature reached was about 230° C., and in that case the sooted globe lost about ten times as much heat as the silvered globe under the same circumstances. When the silvered globe had become tarnished, the radiation from its surface was so much increased that the loss from the sooted globe was only three times that from the tarnished silver.

Table I.—May 18, 1897. Two Platinum Wires, Pt_1 and Pt_2, length 42·55 cm., diameter 0·0542 cm., from the same hank of wire. Pt_1 left with bright surface, Pt_2 thinly sooted.

Current in amperes.	Pt_1.			Pt_2.			Pressure in millimetres.
	Resistance in ohms.	Temperature.	Thermal energy lost per square centimetre per second.	Resistance in ohms.	Temperature.	Thermal energy lost per square centimetre per second.	
0·023	0·192	17	$0·033 \times 10^{-4}$	0·188	17	$0·0329 \times 10^{-4}$	0·00025
0·276	0·208	33	5·198	0·198	26	4·952	0·00025
0·552	0·259	89	25·98	0·209	36	20·96	0·00045
0·695	0·292	130	46·52	0·220	51	35·02	0·00045
0·940	0·337	185	97·97	0·237	71	68·90	0·00045
1·430	0·415	295	279·80	0·271	109	183·1	0·00045
1·937	0·484	453	599·5	0·317	167	393·0	0·00060
2·691	0·570	623	1371·0	0·377	249	912·2	0·00050
3·003	0·599	743	1776·0	0·398	280	1184·0	0·00060
3·770				0·437	343	2055·0	0·00500*
4·446				0·476	414	3106·0	0·00360
5·200				0·515	496	4517·0	0·00320
6·604				0·566	643	8166·0	0·00250

* Owing to increase in the pressure, the emission here must be considerably increased by convection.

Table II.—June 17, 1897. Two Platinum Wires, Pt_3 and Pt_4, lengtl 39·2 cm., diameter 0·025 cm., from the same hank of wire; Pt bright, Pt_4 sooted.

Current in amperes.	Pt_3. Resistance in ohms.	Pt_3. Temperature.	Pt_3. Thermal energy lost per square centimetre per second.	Pt_4. Resistance in ohms.	Pt_4. Temperature.	Pt_4. Thermal energy lost per square centimetre per second.	Pressure in millimetres.
0·0245	1·94	15	$0 \cdot 9004 \times 10^{-4}$	1·94	15	$0 \cdot 9004 \times 10^{-4}$	0·00040
0·0819	2·03	41	10·53	1·984	26	10·29	0·00040
0·1638	2·27	120	48·09	2·050	47	42·52	0·00025
0·2348	2·466	190	105·1	2·134	74	89·00	0·00083
0·3003	2·602	243	181·4	2·224	109	155·1	0·00025
0·3822	2·781	318	314·2	2·314	135	261·5	0·00025
0·4586	2·900	377	473·1	2·399	166	390·2	0·00025
0·5405	3·039	445	686·7	2·492	200	563·0	0·00025
0·6470	3·196	538	1034·0	2·610	245	844·7	0·00016
0·8479	3·418	719	1902·0	2·788	321	1552·0	0·00025
1·0230				2·919	383	2362·0	0·00050
1·1696				3·083	442	3209·0	0·00130
1·462				3·228	560	5338·0	0·0019
1·608				3·331	637	6658·0	0·0025
1·754				3·424	726	8149·0	0·0023

Tables III, IV, V, showing the Amount of Thermal Energy lost pe sq. cm. per second by each of two precisely similar Platinum Wire at the same temperature, one of the wires having a Bright Metalli Surface and the other being Lightly Sooted.

Table III.

Pt_1 and Pt_2. Diameter of Wire, 0·0542 cm.

Temperature.	Energy lost by bright wire.	Energy lost by sooted wire.	Ratio sooted/bright.
200	1·1	5·3	4·8
250	1·8	8·9	4·9
300	2·7	12·8	4·7
350	3·8	19·8	5·2
400	4·9	26·1	5·3
450	6·2	33·3	5·4
500	7·9	42·0	5·3
550	10·0	50·0	5·0
600	11·9	58·5	4·9
650	13·8	68·7	5·0

Table IV.

Pt_3 and Pt_4. Diameter of Wire, 0·025 cm.

Temperature.	Energy lost by bright wire.	Energy lost by sooted wire.	Ratio sooted/bright.
150	0·7	3·3	4·71
200	1·1	5·9	5·4
250	1·8	9·2	5·1
300	2·7	13·5	5·0
350	3·7	18·6	5·0
400	4·9	24·1	4·9
450	6·4	31·0	4·9
500	8·0	38·7	4·84
550	10·1	46·5	4·6
600	12·1	54·0	4·5
650	15·9	67·5	4·2
750	21·8	86·5	4·0

Table V.

Pt_5 and Pt_6. Diameter of Wire, 0·015 cm.

Temperature.	Energy lost by bright wire.	Energy lost by sooted wire.	Ratio sooted/bright.
200	1·0	4·5	4·5
250	1·3	5 7	4·4
300	2·0	8·7	4·35
350	3·2	13·0	4·06
400	4·7	18·8	4·0
450	7·0	27·4	3·9
500	9·9	37·8	3·8
550	14·9	57·0	3·8

FIG. 3.—Curves showing emission of heat from Pt_1 (bright) and Pt_2 (sooted), fro[m] the same hank of wire, diameter 0·0542 cm., at various temperatures.

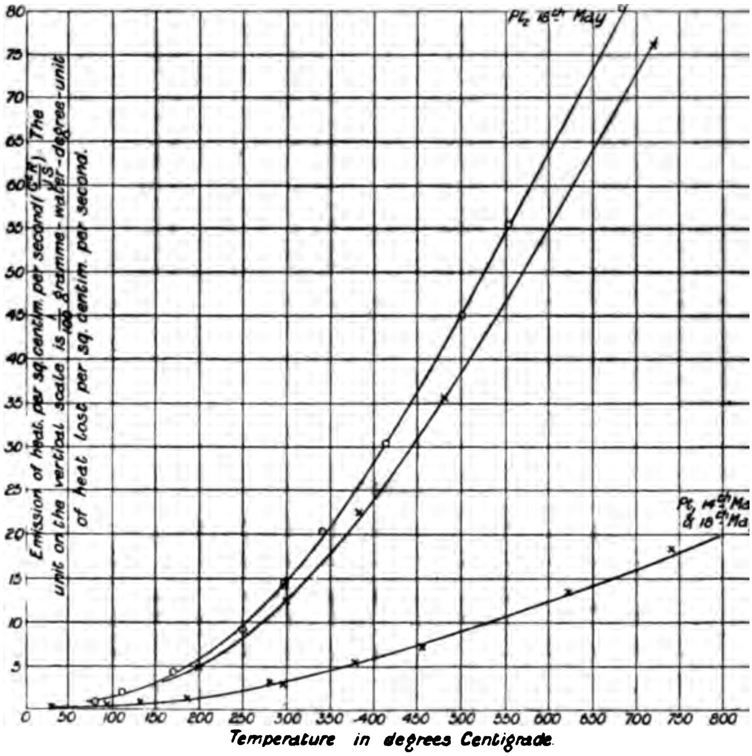


FIG. 4.—Curves showing emission of heat from Pt_3 (bright) and Pt_4 (sooted), from the same hank of wire, diameter 0·025 cm., at various temperatures.

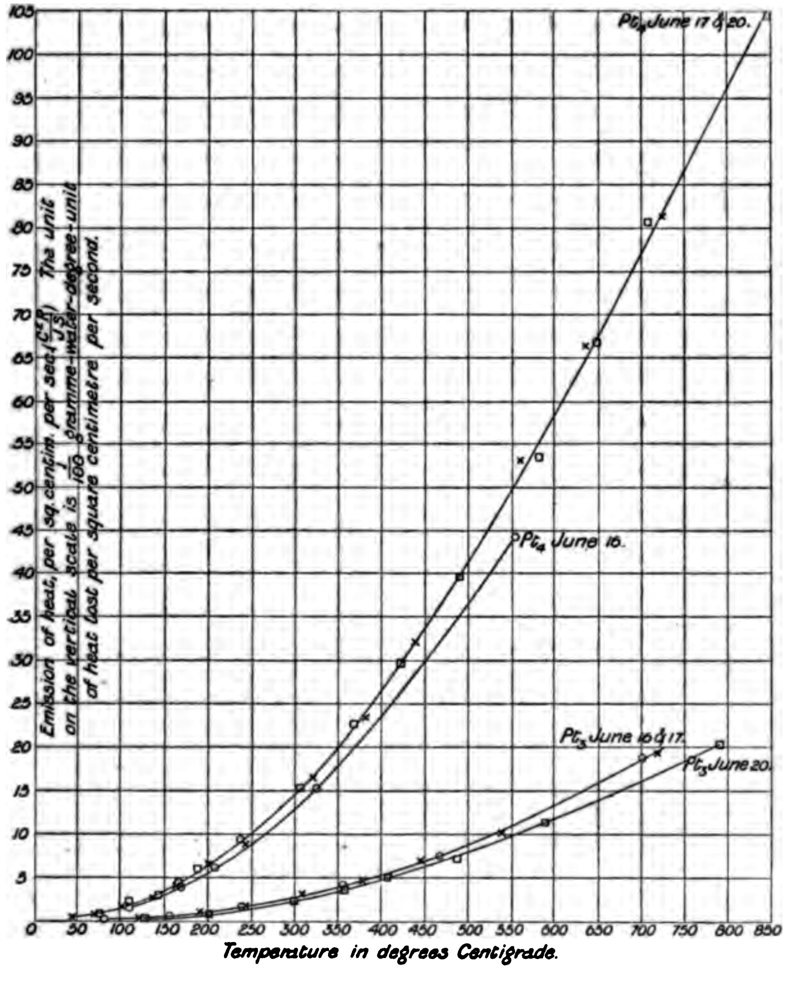


Fig. 5.—Curves showing emission of heat from Pt_3 (bright) and Pt_6 (sooted), from the same hank of wire, diameter 0·015 cm., at various temperatures.

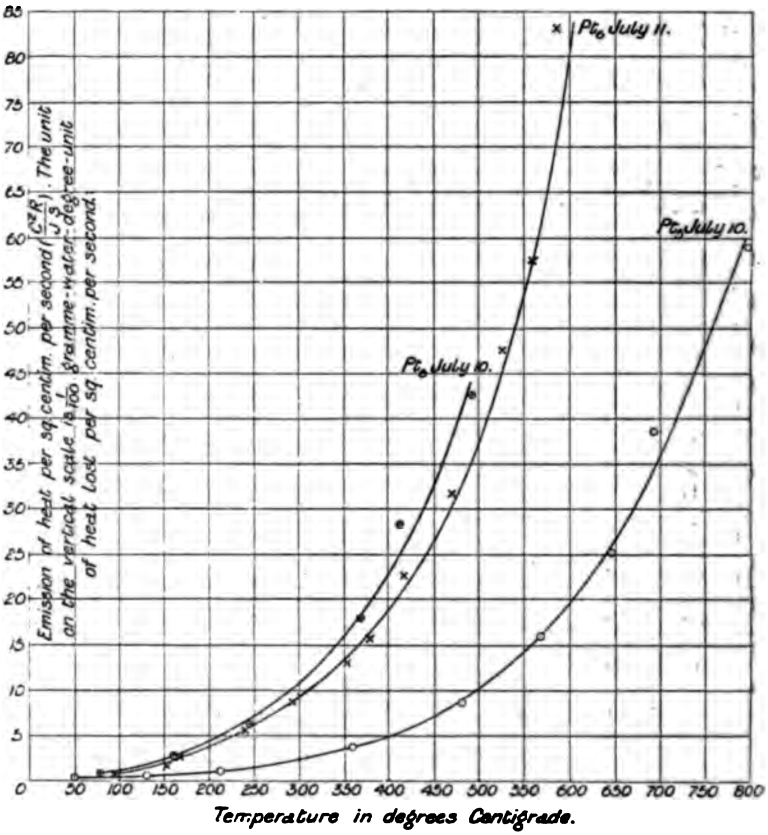


FIG. 6.—Emission of heat from three bright platinum wires, Pt_1, Pt_3, Pt_5, of different diameters as indicated.

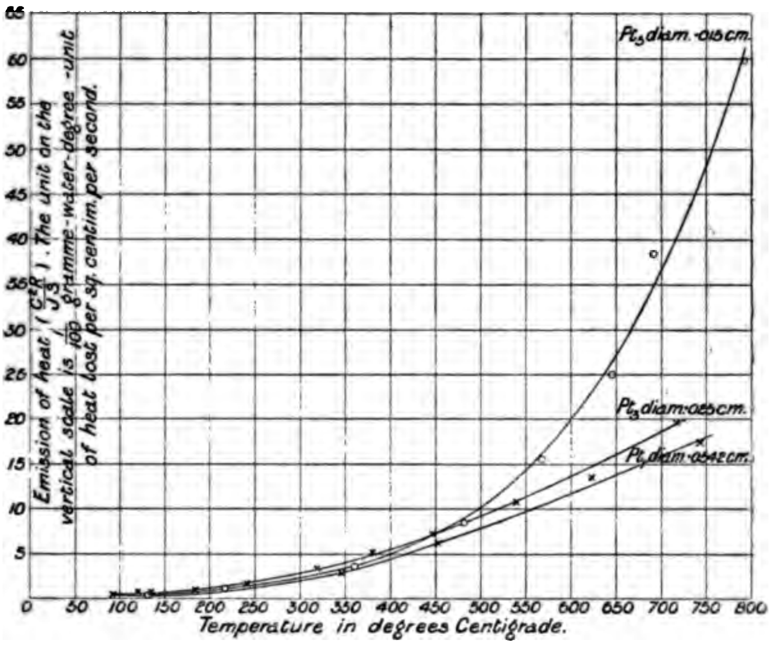
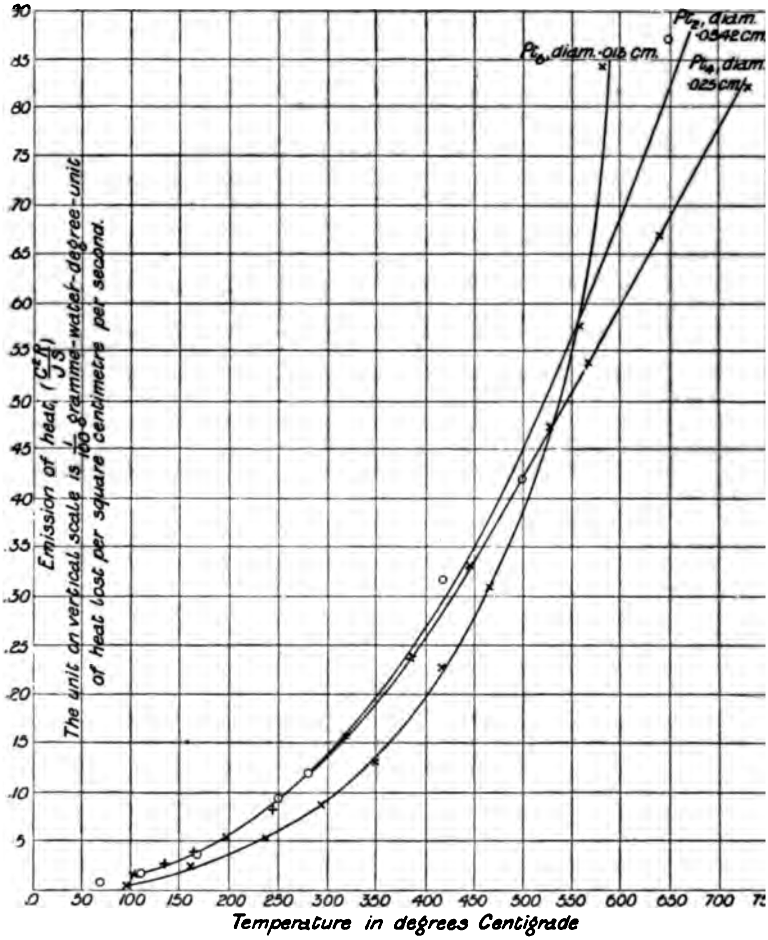


FIG. 7.—Emission of heat from three sooted platinum wires, Pt_2, Pt_4, Pt_6, of different diameters as indicated.



"Photography of Sound-waves, and the Kinematographic Demonstration of the Evolutions of Reflected Wave-fronts." By R. W. Wood, Assistant Professor of Physics in the University of Wisconsin. Communicated by C. V. Boys, F.R.S. Received February 10,—Read February 15, 1900.

In a paper published in the 'Philosophical Magazine' (August, 1899) I gave an account of a series of photographs of sound-waves undergoing reflection, refraction, diffraction, &c., which were made chiefly for the purpose of illustrating certain optical phenomena to classes.

The waves were in every case single pulses in the air produced by electric sparks, illuminated and photographed by the light of a second spark, properly timed with reference to the first, the apparatus being essentially the same as that employed by Toepler for the study of striæ.

I have recently secured, by means of an improved apparatus, a very much better and more complete series of photographs; and at the risk of subjecting myself to criticism for bringing matter already published to the attention of the Society, I wish to devote a few minutes to a very rapid inspection of them.

The following cases, that were not represented in my first paper, I think I may safely comment upon.

The conjugate foci of the elliptical mirror, aplanatic for rays issuing from a point, is very beautifully shown, the spherical wave diverging from one focus being transformed by reflection into a converging sphere which shrinks to a point at the other focus.

The transformation of a spherical into a plane wave by a parabolic mirror is also well shown (fig. 1).

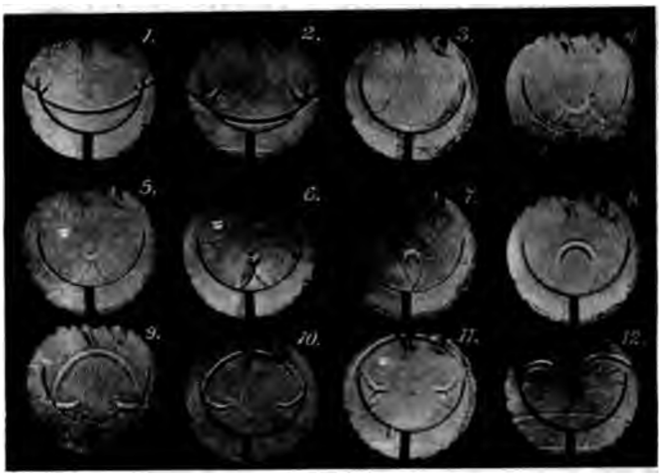
Fig. 1.

The effect of spherical aberration of circular mirrors is beautifully exhibited in several cases.

When a plane wave enters a hemispherical mirror the reflected wave front is cusped, and the cusp will be seen to lie always on the caustic surface. The form of the complete wave in this case is not unlike a

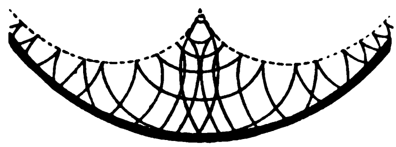
volcanic cone with a bowl-shaped crater, the bowl eventually collapsi to a point, at the focus of the mirror, the sides of the cone running under it and crossing. From now on the wave diverges, and goes (of the mirror in a form somewhat resembling the bell of a medusa, t caustic form by twice-reflected rays being traced by a second cı (fig. 2).

Fig. 2.



These forms can, of course, be constructed geometrically, and have here a slide with a number of successive positions of the wa front, showing how the cusps follow the caustic surface (fig. 3). T

Fig. 3.

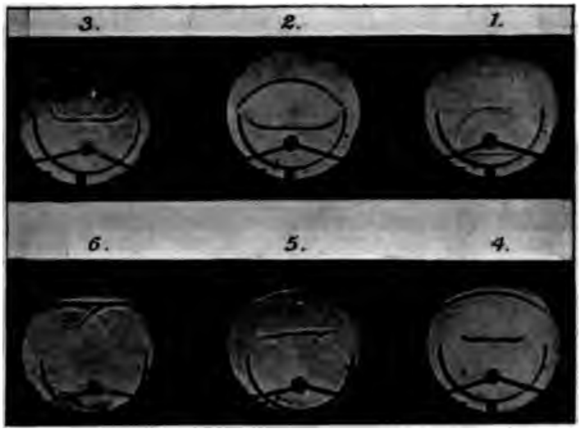


construction shows that there is a concentration of energy at the cus consequently we may define the cusp as a moving focus, and the caus as the surface traced by it. Though I hesitate in claiming that this re tion, at once so apparent, is at all novel, I may say that, so far a have been able to find, it is not brought out in any of the text-boo caustic surfaces being invariably treated by *ray* rather than by *wa front* methods.

If the wave starts at the principal focus of a hemispherical mirr the reflected front is nearly plane in the vicinity of the axis, curli up at the edges, however. As this flat-bottomed saucer moves up, t

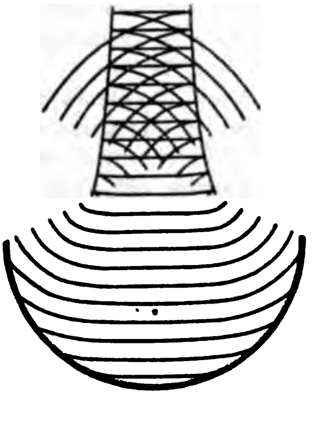
curved sides come to a focus along the circular edge of the flat bottom, so that in one position the front appears as a true plane (fig. 4); but

Fig. 4.



from this point on the curved sides, having passed through a focus, diverge again and follow the flat bottom. The cusp formed by the union of these two portions traces the caustic surface, which is in this case a very tapering funnel, as is shown well by the geometrical construction (fig. 5).

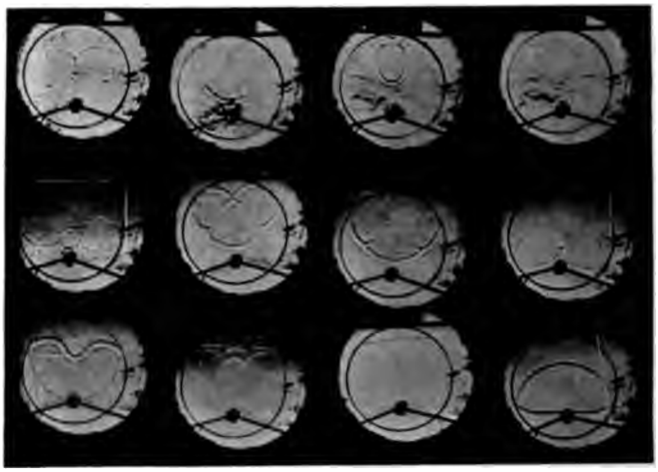
Fig. 5.



If now we substitute for the hemisphere a complete sphere, we obtain very complex forms which cannot be followed except by geometrical constructions, for the wave is shut up in the mirror and reflected back and forth, becoming more complicated at each reflection (fig. 6). That all of these very intricate forms can be constructed by

geometry I shall show presently; and by means of the animatograph, which Mr. Paul has most kindly placed at our disposal, we can actually see the wave going through its gymnastics.

Fig. 6.



The principle of Huygens, that any small portion of a wave-front can be considered as the centre of a secondary disturbance, and that a small portion of this new disturbance can in turn be regarded as a new centre, can be shown by the sound-waves, as well as the obliteration of the shadow by diffraction, and the secondary wavelets reflected from corrugated surfaces, interesting in connection with the diffraction grating.

Various cases of refraction are also shown, the only novel one being the transformation of a spherical into a plane wave by a carbonic-acid lens. The construction of the cylindrical lens, of exceedingly thin collodion, a matter of considerable difficulty, was successfully accomplished, the circular flat ends of very thin mica, free from striæ, enabling the passage of the wave through the lens to be followed (fig. 7). The other cases of refraction have already been described, as

Fig. 7.

well as the very beautiful instance of the formation of a train of waves, or musical note, by the reflection of a single pulse from a steep flight of steps (fig. 8).

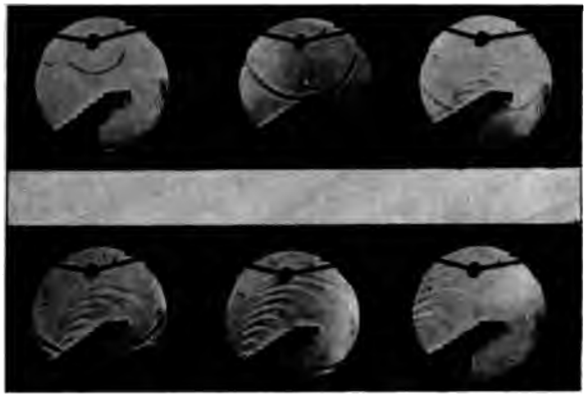


FIG. 8.

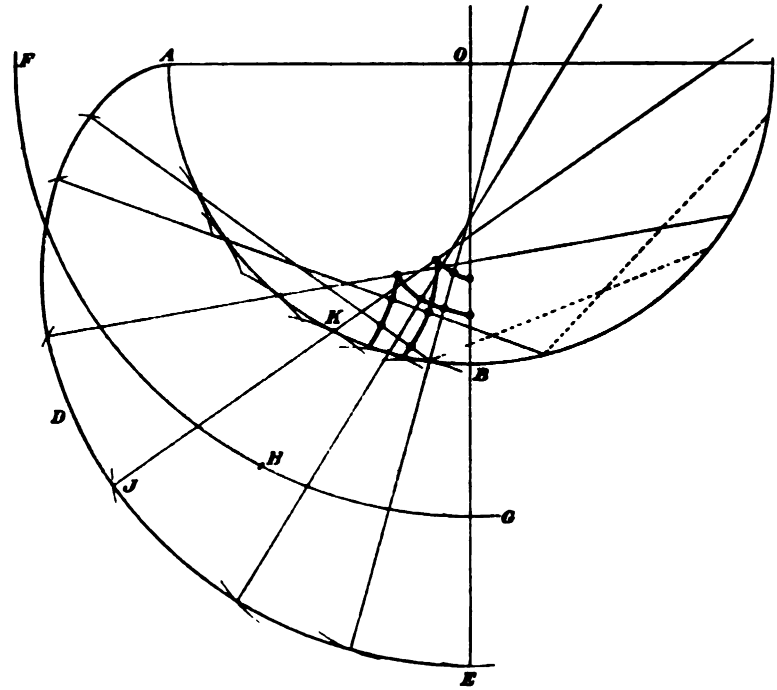
Returning now to the evolutions of plane and spherical waves after reflection from spherical surfaces, I wish to bring to the attention of the Society a method of demonstrating in a most graphic manner the progressive changes in the wave-front reflected under these conditions.

Having been unable to so control the time interval between the two sparks that a progressive series could be taken, I adopted the simpler method of making a large number of geometrical constructions, and then photographing them on a kinetoscope film.

As a very large number of drawings (100 or so) must be made if the result is to be at all satisfactory, a method is desirable that will reduce the labour to a minimum. I may be permitted to give, as an instance, the method that I devised for building the series illustrating the reflection of a plane wave in a spherical mirror. The construction is shown in the figure.

ABC is the mirror, AOC the plane wave. Around points on ABC as centres describe circles tangent to the wave. These circles will be enveloped by another surface, ADE, below the mirror (the orthogonal surface). If we erect normals on this surface, we have the reflected rays, and if we measure off equal distances on the normals, we have the reflected wave-front. By drawing the orthogonal surface we avoid the complication of having to measure off the distances around a corner. The orthogonal surface is an epicycloid formed by the rolling of a circle of a diameter equal to the radius of curvature of the mirror on the mirror's surface, and normals can be erected by drawing the

arc FG (the path of the centre of the generating circle), and describing circles of diameter BE around various points on it. A line joining the point of intersection of one of these circles with the epicycloid, and the point of tangency with the mirror, will, when produced, give



a reflected ray; for example, JK produced for circle described around H. This construction once prepared, the series of wave-front pictures can be very quickly made. Three or four sheets of paper are laid under the construction and holes are punched through the pile by means of a pin, at equal distances along each ray (measured from the orthogonal surface).

The centre of the mirror and the point where its axis meets the surface are also indicated in the same manner. The sheets are now separated, and corresponding pin-holes are united on each sheet by a broad black line, which represents the wave-front. After a time it becomes necessary to consider double reflections, and to do this we are compelled to construct twice-reflected rays (indicated by dotted lines), and measure around a corner each time.

About a hundred pictures are prepared for each series, and the pictures then photographed separately on the film, which, when run

Fig. 9.

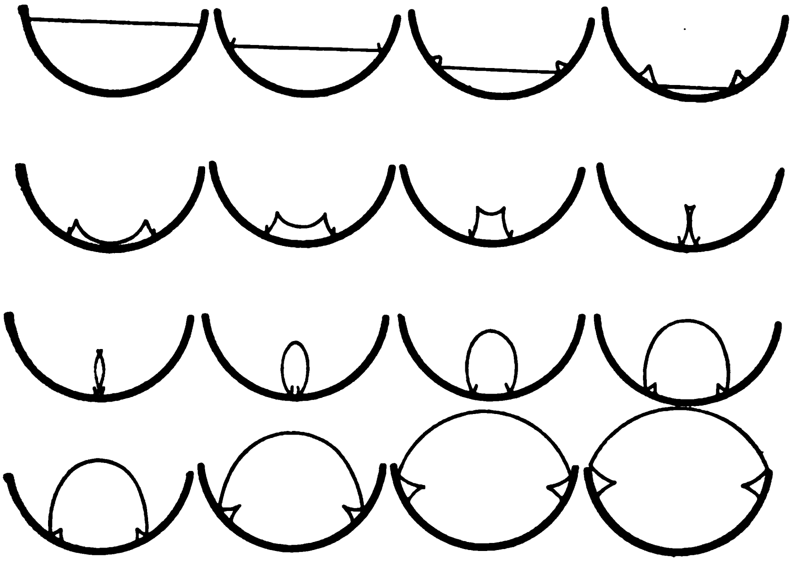
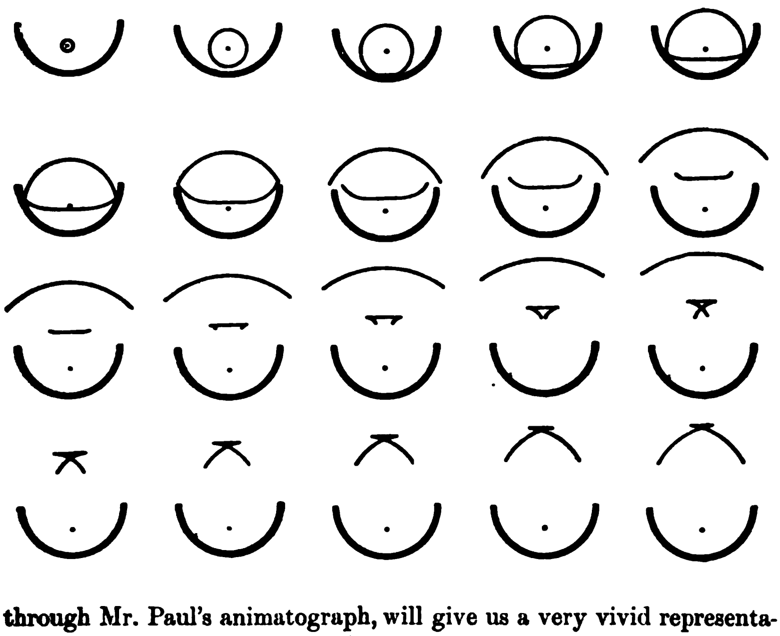


Fig. 10.

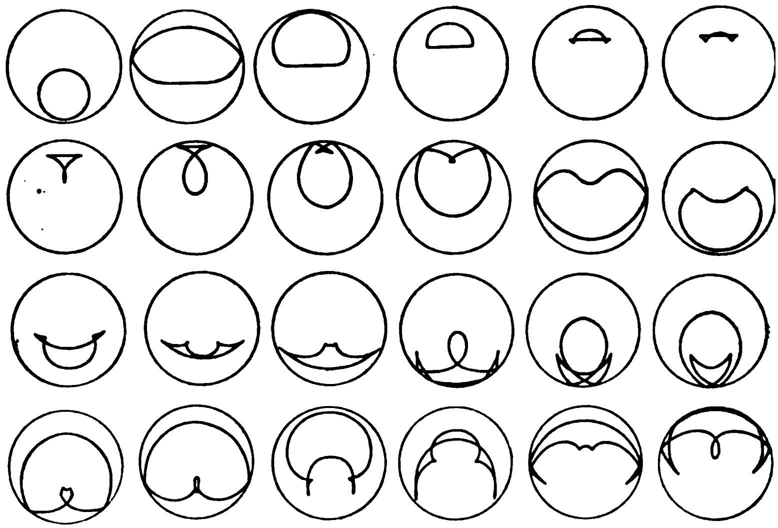


through Mr. Paul's animatograph, will give us a very vivid representation of the motion of the wave-front.

The series illustrating reflection inside of a complete sphere was the

most difficult to prepare, as several reflections have to be considered. It has been completed for three reflections, and Mr. Max Mason, of Madison, to whom I am greatly indebted for his patient work in assisting me, is going on with the series. As will be seen, the wave has already become quite complicated, and it will be interesting to see what further changes result after three or four more reflections. I am also under obligations to Professor A. B. Porter, of Chicago, who prepared the set of drawings illustrating the passage of a wave out from the principal focus of a hemispherical mirror.

FIG. 11.



A number of points taken at intervals along the film are here reproduced, and give a fair idea of the transformations. Fig. 9 shows the plane wave entering the hemispherical mirror, while in fig. 10 we have a spherical wave starting on the principal focus of a similar reflecting surface (compare fig. 9 with fig. 2, and fig. 10 with fig. 4). Fig. 11 shows the evolutions of the wave shut upon the complete spherical mirror, and shows the development of the complicated photographed forms shown in fig. 6.

"*Polytremacis* and the Ancestry of Helioporidæ." By Professor J. W. GREGORY, D.Sc. Communicated by Professor RAY LANKESTER, F.R.S. Received November 21,—Read December 7, 1899.

[PLATE 2.]

CONTENTS.

1. INTRODUCTION.

The Blue Coral, *Heliopora cærulea* (Pall.) is one of the most isolated of living animals. It is the only known species of its genus, and it has recently been described as the only member of its family. Some Palæozoic corals have a very similar structure; but the view that these extinct Heliolitids are allied to the Helioporids is strongly opposed by some eminent palæontologists. If these authorities be right, then *Heliopora* is an animal with no close living relations and with no known ancestors.

The only fossil that has been regarded with any probability as a possible link between *Heliopora* and the extinct Heliolitidæ is the Cretaceous coral *Polytremacis.* This genus was founded by d'Orbigny in 1849, but unfortunately its affinities and structures are still in doubt. "If a genus ever was in need of revision," recently exclaimed Professor Lindström [1899, 9, 28], "it is this." Lindström indeed suggests that distinct genera are required for each of von Reuss' three species of *Polytremacis.*

Without a study of type-specimens in several Continental museums, a final revision of the genus is impossible for three reasons. The characters of *Polytremacis bulbosa* d'Orb., the species on which the genus was originally established, are quite unknown; there is one uncertain feature in *P. blainvillei* (Mich.), the acting type species; and doubts have been expressed as to the accuracy of von Reuss' figures of the specimens which he identified as *P. blainvillei.* But in preparing a description of a new species of *Heliopora* from Somali-land, I have been led to examine the material in the British Museum collection. The

results seem to confirm the old view of the affinity between the Heliolitidæ and the Helioporidæ, by showing that *Polytremacis* is truly intermediate between the two families. In that case *Polytremacis* is of considerable phylogenetic interest as an ancestor of *Heliopora*. I therefore venture to submit this paper to the Society which has published the two most important contributions to our knowledge of that important coral.

2. The Type of Polytremacis.

Polytremacis was founded by d'Orbigny in 1849, when he gave it the following very inadequate diagnosis [**14**, p. 11]:—" c'est un *Stylophora* sans saillies aux calices, ceux-ci simplement creusés. Intervalle d'un tissu poreux, granuleux en dessus. Ensemble amorphe." The only species named is *P. bulbosa* d'Orb. from the Cenomanian of the Ile d'Aix, Charente-Inférieure; it was defined the following year [**15**, p. 183] as " espèce globuleuse, arrondie à calices assez grands." That species has never been figured, and the type specimen is apparently not available, as Milne Edwards and Haime [**5**, p. 232] who quoted it, have simply repeated d'Orbigny's statement. *P. bulbosa* may therefore be dismissed as a *nomen nudum*, and another type must be found for the genus. This task is easy. D'Orbigny's second reference to *Polytremacis* was in his 'Prodrome de Paléontologie,' where (vol. 2, p. 209) he gives a list of four species. The first of the four is the *Heliopora blainvillei* of Michelin [**10**, p. 27, Plate 7, fig. 6], from the Turonian of Vaucluse. The other three were new species founded by d'Orbigny, apparently on mere varieties of *P. blainvillei*. The three new species were not figured, and were subsequently accepted by Milne Edwards and Haime on d'Orbigny's authority. There can therefore be no question that of these four species *P. blainvillei* (Mich.) must be taken as the type of *Polytremacis*.

The characters of this species are, however, somewhat doubtful. Michelin's original figure represented a lobed corallum, with one short cylindrical branch; the calicles are generally crowded and separated by areas about as wide as themselves. The rim of the calicle is notched by a series of sixteen or twenty teeth.

In 1854 von Reuss [**16**, p. 136, Plate 24, figs. 4–7] figured a coral from the Turonian of Gosau, and identified it as *P. blainvillei*. Milne Edwards and Haime [**5**, p. 232], to whom Michelin's types were easily accessible, accepted the accuracy of the determination, and described von Reuss' figures as " very good." But Lindström not only denies the accuracy of the specific identification, but urges that the coral is generically distinct from *Polytremacis* [**9**, p. 28].

The *Polytremacis blainvillei* of Michelin differs from that of von Reuss in two respects. The coral thus named by the former author has no lamellar septa, but only a series of " pseudosepta " or septal

teeth; in the coral figured by von Reuss, each calicle has from eight to fourteen lamellar septa. The second difference is much less important, and consists in the more crowded arrangement of the calicles in Michelin's type of the species; but the distribution is not uniform in Michelin's specimen, and varies enormously in a series of specimens from the Turonian of the Bouches-du-Rhône recently received by the British Museum. The difference in the septa is, however, more significant; that the septa in some of the Gosau *Polytremacis* are lamellar is shown by Lindström's own description of a specimen sent him from Vienna. It does not seem to me necessary to regard the difference as of generic value; but it certainly seems reasonable to treat it as a specific distinction, and I therefore propose to name the Gosau specimens with long septa *Polytremacis septifera.*

The type species of *Polytremacis* is therefore *P. blainvillei* (Mich.) *non* Reuss.

3. The Structure of Polytremacis.

Corallum.—The corallum is irregularly lobed, or grows in thick cylindrical branches. The whole surface is granulated. The calices are crowded (*P. blainvillei*) or widely and irregularly separated (*P. partschi*). The greatest separation is due to the closure of dead calicles by growth of cœnenchymal cæca (as in *Heliolites interstinctus*; Lindström, **9**, Plate 1, fig. 21). This closure is illustrated by a figure of two calicles of *P. macrostoma* (Plate 2, fig. 1).

Thin sections show that the corallum is deeply excavated by large cylindrical calicles, the walls of which are smooth or fluted. In typical calicles the walls are thick; but young calicles and some internal ones may remain in a thin-walled *Heliopora* stage (Plate 2, fig. 3). The calicles are surrounded by narrow cæca, which are circular or elliptical in section. The cæca may be irregular in arrangement, or occur in a circle round a calicle. Outgrowths from the cæca or from the calicle traverse the cæcal mass like canals.

Septal Structures or "*Pseudosepta.*"—The external rim of the calicle is marked by an irregular series of granules forming septal teeth like those of *Heliopora*, as, *e.g.*, in Michelin's original figure of *P. blainvillei.* These teeth may be continued down the sides of the calicles as continuous ridges, which may be few and long, as in *P. septifera*, or numerous and short, as in *P. blainvillei.* The septal ridges may be continued radially outwards; on the surface they then appear as lines of radial granules (Plate 2, fig. 1); internally, in thin sections, they appear as lamellæ, continued outward as costal lamellæ separating the cœnenchymal cæca. This arrangement is not shown in all sections; it is illustrated by Plate 2, fig. 4A. Lindström's figure of *Plasmopora suprema* shows a similar structure [**9**, Plate 7, fig. 24].

In the older parts of the calicles the septal structures are absent

and the calicular walls are plain, as in the corresponding stages of *Heliopora* (*cf.* **7**, Plate 13, fig. 4) and some *Heliolites* [**9**, Plate 2, figs. 16, 18, and 20].

The number of septal ridges and teeth is variable (as in *Heliopora*) the number is from 16 to 20 in *P. blainvillei* and from 8 to 20 in *P. septifera*.

Aureole.—The wall of the calicle may be thin in young and some internal calicles, but in mature calicles it is greatly thickened. It may be surrounded by an "aureole" (Lindström) of large cæca, with the walls continuous with the septa as in the Heliolitid *Plasmopora* (*cf* Lindström's figure of *P. stella*, **9**, Plate 11, fig. 36).

Tabulæ occur across both the calicles and cœnenchymal cæca.

Baculi.—Rod-like pillars of compact, calcareous material, which Lindström has described in the Heliolitidæ, occur in *P. blainvillei*, as remarked by Lindström [**9**, p. 28].

4. The Affinities of Polytremacis.

A. *The Relations of the Helioporidæ and Heliolitidæ.*

The preceding account of the structure of *Polytremacis* shows that the coral consists of a series of tubes, which are marked internally by longitudinal ridges, are crossed by transverse tabulæ, and are separated by smaller cæcal tubuli. This structure agrees with that of both the living Helioporidæ and the Palæozoic Heliolitidæ, and the affinities of *Polytremacis* are clearly with one or other of those families.

We have, therefore, to consider the question whether the two families are themselves nearly related. All the older and many recent authorities regard them as intimately allied. Blainville, in 1834 [**2**, p. 392], included them both in the genus *Heliopora*. Dana in 1848 [**4**, pp. 539–541], separated them generically, but left them in one sub-family—the Helioporinæ. Zittel, in 1879 [**20**, p. 212] Studer, in 1887 [**18**, p. 21], and Sardeson, in 1896 [**17**, p. 353], al included them in one family. Bourne, in 1895 [**3**], has warmly supported the view of the intimate alliance of the two groups. On the other hand the existence of any special affinity between the Heliopor̄ids and the Heliolitids is denied by Lindström [**9**, pp. 25–26] Hinde [**8**, p. 87], and Wentzel [**19**, p. 490]. It is not even always admitted that the corals belong to the same subphylum; for while *Heliopora* is unquestionably an Alcyonarian, according to F. Bernard [**1**, p. 187], the Heliolitidæ may be Hydrozoa.

The proposed separation of the Helioporidæ from the Heliolitidæ is based on two characters: (1) the presence of true septa in the latter and not in the former; (2) the absence from the Helioporidæ of the calicular theca of the Heliolitidæ.

To determine the affinities of *Polytremacis* we must appreciate these characters:—

B. *The Septal Structures.*—According to Dr. Hinde [**8**, p. 87], Neumayr [**11**, p. 320–1], and J. Wentzel [**19**, p. 490], *Heliolites* differs essentially from *Heliopora*, in the possession of definite septa, which those authors apparently regard as homologous with the septa of madreporarian corals. In *Heliopora* there are no such septa; the structures originally described as such are a series of teeth round the rim of the calicular tube; below each tooth a fluted ridge runs down the tube for some distance. Neumayr, in 1899, proposed for these ridges the name of "pseudosepta" [**11**, p. 306], and the term has been widely accepted; for, as Lindström remarks, the ridges are simply the projections of the cœnenchymal cæca.*

But the rule that the Heliolitids have septa and *Heliopora* has only "pseudosepta," is not absolute. Lindström has figured sections across *Heliolites* in which the septa are absent and the sections are identical with those of *Heliopora*. For instance, Lindström's figure [**9**, Plate 1, fig. 24] may be compared with a section of *Heliopora cærulea* figured in 1895 [**7**, Plate 63, fig. 4]. Both sections consist of crowded polygonal, thin-walled tubes, without any sign of septa or "pseudosepta." Moreover, Nicholson has figured a calicle of *Heliopora* in which the "pseudosepta" are more strongly developed than in the "septa" of some *Heliolites* [**12**, p. 333].

But it is by no means certain that there is any essential difference between the septa of *Heliolites* and the "pseudosepta" of *Heliopora*. According to Bourne the large calicles of *Heliopora* are formed by the fusion of nineteen cœnenchymal cæca into a single cavity. The fusion of the group of cæca is caused by the expansion of the central cæcum, which, as it grows, absorbs the adjacent parts of the surrounding cæca. The outermost parts of the walls between the six peripheral cæca remain for a time as radial septa; they are finally absorbed as the central cavity increases, and when it occupies the whole space of the group it is bounded by a plain wall. The various stages in this process may be seen along the growing edge of a lamellar corallum of *Heliopora*. It is illustrated by a series of four figures. Fig. 6*a* shows a group of tubes of which the central member is slightly larger than those of the surrounding series. There are no septa or septal ridges, and the arrangement is identical with that of a young *Heliolites* in the

* The structures are here described as septa, using the term in its descriptive sense. When the septa are greatly reduced they are referred to as septal ridges, analogous to the septal spines of Madreporaria. If the pejorative prefix be accepted in the one case, it ought to be in the others, and *Polytremacis* might be defined as a coral (or perhaps a pseudocoral) composed of pseudotheca, with a variable number of pseudosepta, separated by pseudocœnenchyma, traversed by *pseudotabulæ*, and with a basal deposit of pseudepitheca.

stage before the development of septa. The next figure (fig. 6*b*) shows a slight increase in the size of the central tube and reduction in that of the peripheral tubes. In the next stage (fig. 6*c*) the central tube is large, and is surrounded by a zone of compressed tubuli. Finally, there is the stage in which the septal structures appear. This stage is illustrated (fig. 6*d*) by a calicle with one well-developed septum, which is the continuation of the wall separating two adjacent tubuli.

That the calicles and septa of *Heliolites* are formed by the same process appears probable from evidence cited by Lindström, who has given a series of figures showing the development of a group of cæca into a large calicle, some of the cæcal walls remaining as septa (see Plate 2, fig. 7, *a–g*).

Hence it appears probable that the septa of *Heliolites* are not homologous with the septa of Madreporaria; for they are the remnants of walls and not special outgrowths from the margin of corallites. They are as much "pseudosepta" as the corresponding ridges in *Heliopora*. Why the septal structures are, as Professor Nicholson remarks, "approximately constant" in number and large in *Heliolites*, while they are small in size and variable in number in *Heliopora*, is easily explained. It is, in fact, the necessary consequence of the difference in size and regularity of the cœnenchymal cæca in the two genera. The cæca of *Heliopora* are relatively more numerous, smaller, and less regular than in *Heliolites*. Accordingly, as the calicle of *Heliopora* grows and absorbs the surrounding cæca, there is left a considerable and variable number of septal ridges.

That the cæca of the modern representatives of the Heliolitidæ should be smaller than those of the Palæozoic forms is not surprising. It is the natural line of development. *Heliopora* may therefore be explained as a Heliolitid in which the cæca have decreased in size and increased in number.

C. *The Calicular Theca of Heliolitidæ*.—According to Lindström, "the feature which decidedly removes it [*Heliopora*] far from the Heliolitidæ is the total want of a calicular theca." Professor Frech gives different expression to the same idea; he states [**6**, p. 500] that the walls of the calicles in *Heliopora* are perforated and incomplete, whereas those of *Heliolites* are complete, and the calicles are closed tubes. Frech's statement is, however, not correct, as a matter of fact [*cf.* Moseley, **13**, p. 112]; but his idea is apparently the same as that which has been so beautifully worked out by Professor Lindström.

Heliolites, according to Lindström, has a true theca,* which is the

* It may save some misunderstanding to remark that Lindström distinguishes three thecal structures: (1) the calicular theca which bounds the inner axial part of the calicle; (2) the external theca, which includes the calicular theca and all the cœnenchyma which has developed from it; and (3) the cœnotheca (*non* Bourne), which covers the lower part of the corallum like the epitheca of compound Madreporaria.

first part of the skeleton to be formed, and which persists in the adult as the calicular tube or inner tube of the calicle. In the development of a young *Heliolites* the thecal tube is first formed; when this tube is complete a series of septa develop from the inner walls of the tube, and then the cœnenchyma begins to form on the outer side of it.

Bourne, on the other hand, gives a very different explanation of the structure of the corallum, and holds that it is fundamentally the same in *Heliopora* and *Heliolites*, in both of which the calicle is bounded by a "cœnotheca," *i.e.*, a tube formed of the walls of a group of different elements in a colony, secondarily united into a single tube [**3**, p. 468].

Unfortunately nothing is known of the development of the primary calicle in *Heliopora*, so that no direct comparison of that stage in the two groups is possible. But the comparison of the formation of young calicles on the growing edges of *Heliopora* affords some suggestive hints. The young calicles in both genera pass through identical stages, which are represented for *Heliopora* by fig. 6, *a–d*, and for *Heliolites porosus* by fig. 7, *a–g*. In both cases the calicular theca of the complete calicle represents either the outer walls of the group of cæca which formed the calicle, or was formed by those outer walls being absorbed and re-deposited during the process of cœnenchymal gemmation.

A direct comparison of the development of the primary calicles in *Heliopora* and *Heliolites* would, no doubt, afford a better basis for an opinion than can be obtained from the development of young calicles in old coralla. But until zoologists work out the development of *Heliopora*, we can only appeal to the comparison of young equivalent calicles, and they develop on the same lines.

Hence, though nervous at differing from two such authorities as Professor Lindström and Dr. Hinde, I am bound to confess myself unconvinced that any essential difference between the Helioporidæ and Heliolitidæ has yet been established. Accordingly it is not unreasonable to expect in Mesozoic deposits some connecting links between the living and Palæozoic representatives of the group. *Polytremacis* appears to me to be such an intermediate form. *P. septifera*, with its eight to fourteen or twenty well-developed septa, agrees with *Heliolites*, differing by the less regularity in the number of septa. The Turonian *P. blainvillei* and the Eocene *P. bellardi* agree with *Heliopora*, as the septa are reduced to septal ridges.

If we place any species of *Polytremacis* in the Heliolitidæ,* that family can no longer be described as characterised by the possession of twelve septa. If, on the other hand, we place *P. septifera* in the Helioporidæ, we have to admit in that family the presence of septa as well-defined as they are in some Heliolitidæ. In either case the distinction between the two families, based on the septal characters,

* As suggested by Neumayr, 11, p. 321.

has broken down. The only escape from this difficulty is the heroi course of dividing *Polytremacis* into two unrelated divisions, one specie being regarded as an isolated, belated survivor of the Heliolitidæ, an another as a premature ancestor of the Helioporidæ.

Polytremacis agrees with the Heliolitidæ by many remarkable point of structure, such as the presence of the aureole, the closure of dea calicles by cœnenchymal overgrowth, and the inconstancy of the sept in the lower parts of the calicles. *Polytremacis* is allied to *Heliopora* b equally striking points of resemblance, such as the fluted calicula walls, with their numerous, irregular, septal ridges, the granula external surface with its circumcalicular ring of septal teeth. On th axiom that things that are allied to the same are allied to one anothe the close affinity of *Heliopora* and *Heliolites* seems more probable tha some palæontologists are inclined to admit. *Heliopora*, in fact, ma have descended from the Heliolitidæ by the reduction in size and conse quent increase in number and in variability of arrangement of th cœnenchymal cæca.

5. Systematic Synopsis.

ALCYONARIA.

Order.—Cœnothecalia, Bourne.

Family 1.—Heliolitidæ.

Definition.—Cœnothecalia with regular, well-developed septa, gene ally 12 in number in each calicle.

For subdivisions, see Lindström [**9**, pp. 35–37].

Family 2.—Helioporidæ.

Definition.—Cœnothecalia, with small, irregularly arranged cœne chymal cæca, and a variable number of septa or septal ridges.

Genus 1.—Heliopora, de Blainville, 1834.

Definition.—Corallum of thick lobes or digitate fronds. Calicles thi walled. Septal ridges numerous, and always short.

Type Species. Millepora cærulea, Pallas, 1766. 'Elench. Zooph. p. 256.

Heliopora cærulea, Blainville, 1834. 'Man. Act.,' p. 392.

Recent. Indian Ocean.

Species 1.—Heliopora somaliensis, n.sp.

Characters.—Corallum massive. Calicles very small, being betwee 0·5 and 1 mm. in diameter. They are about 2 mm. apart. From 12–15 septal ridges, which are sometimes prominent and well developed. Cœnenchymal cæca circular. (Plate 2, figs. 8a–c.)

Distribution.—Turonian, Uradu limestone. Uradu near the Rugga Pass, Somaliland. (The Rugga Pass is in 45° 22′ E. and 10° N.) Collected by Mrs. E. Lort Phillips. Type in British Museum, R4150.

Affinities.—The nearest ally of this species is *H. edwardsi* Stol., from which it differs by having smaller and less distant calicles, and some areas of angular pseudocæca.

Species 2.—Heliopora edwardsi Stoliczka, 1873. 'Pal. Ind., Cret. Fauna, S. India,' vol. 4, Part IV, p. 53, Plate 11, fig. 11.

Characters.—Corallum incrusting; calicles 1 mm. in diameter, and from 4 to 5 mm. distant. Cœnenchymal cæca numerous and minute. Septal ridges 18 in number.

Stoliczka describes this species as almost identical with *Heliopora cærulea.*

Distribution.—Cenomanian. Utatur Group. E. of Kauray, S. India.

Species 3.—Heliopora boettgeri. Fritsch, 1878. "Kor. Nummulitensch. Borneo," 'Palæontogr.,' Suppl. III, Lief. 1, Part III, p. 103, Plate 17, fig. 4.

Characters.—Corallum a thin incrustation, 1 to 4 mm. thick. Calicles 1 mm. in diameter. Septal ridges, 16–24. Calicles widely spaced, with small, round cæca.

Distribution.—Eocene. Borneo.

Genus 2.—Polytremacis, d'Orbigny, 1849.

Synonyms—

Heliopora, pars, Michelin, Edwards and Haime, *et alii.*

Dactylacis, d'Orbigny.

Definition.—Helioporidæ with thick-walled calicles.

Type Species.—*Heliopora blainvillei*, Michelin, 1841. 'Icon. Zooph., p. 27, Plate 7, fig. 6.

Affinities.—This genus is accepted by Milne Edwards and Haime, von Reuss, and Stoliczka for the Helioporids with long septa, which almost meet in the middle of the calicles. But von Reuss has remarked on the difficulty in using this uncertain character, and concludes that the separation of the genera is not based on any very firm ground.

The character of the calicular walls appears more reliable, especially as it appears to be geographically distinctive, *Heliopora* being limited to the Indian Ocean and Pacific, while *Polytremacis* occurs in the Upper Cretaceous and Lower Cainizoic of central Europe and France. The two genera seem very closely allied, and I should not be surprised if they are ultimately united.

Species 1.—POLYTREMACIS BLAINVILLEI (Michelin), 1841.

Synonymy,—Heliopora blainvilliana, Michelin, 1841. 'Icon. Zooph., p. 27, Plate 7, fig. 6.
non „ „ Quenstedt, 1852, 1867, 1880, and 1885.
Polytremacis „ d'Orbigny, 1850. 'Prod. Pal.,' vol. 2, p. 209.
„ „ Milne Edwards and Haime, 1851. "Polyp. palæoz.," 'Archiv. Mus. Hist. nat.,' vol. 5, p. 149.
non „ *blainvilleana*, von Reuss, 1854. "Kreid. Ostalp.," 'Denk. Akad. Wiss. Wien,' vol. 7, p. 131, Plate 24, figs. 4–7.
„ „ *pars*, Milne Edwards and Haime, 1860. 'Hist. nat. Cor.,' vol. 3, p. 232.
„ „ *pars*, Sardeson, 1896. "Bezieh. Tabul.," 'N. Jahrb.,' Beil. Bd. 10, pp. 261–262.
„ „ Lindström, 1899. "Heliolitidæ," 'Handl. k. Svensk. Vet.-Akad.,' vol. 32, No. 1, p. 28.

Characters.—Calicles very crowded over most of the corallum. Calicles 1–1·5 mm. in diameter. Septal ridges from 16 to 22; very short.

Distribution.—Turonian. Uchaux, Vaucluse.

Species 2.—POLYTREMACIS PARTSCHI, von Reuss, 1854.

Synonymy,—Polytremacis partschi, von Reuss, 1854. "Kreid. Ostalp.," 'Denk. Akad. Wiss. Wien.,' vol. 7, p. 131, Plate 24, figs. 1–3.
„ „ Sardeson, 1896. "Bezieh. Tabul.,' 'N. Jahrb.,' Beil. Bd. 10, p. 260.
Heliopora „ Milne Edwards and Haime, 1860. 'Hist. nat. Cor.,' vol. 3, p. 231.
„ „ von Zittel, 1879. 'Handb. Pal.,' vol. 1, p. 212, fig. 122.
„ *blainvilliana*, Quenstedt, 1852. 'Handb. Petref.,' p. 645, Plate 57, fig. 8.
„ „ Quenstedt, 1867. *Op. cit.*, edit. 2, p. 775, Plate 74, fig. 8.

Synonymy,—Heliopora blainvilliana, *pars*, Quenstedt, 1880. 'Petref. Deut.,' vol. 6, Part 11, p. 901, Plate 178, fig. 30, *non* 30*x*.
„ „ Quenstedt, 1885. 'Handb. Petref.,' p. 997, Plate 80, fig. 28.

Characters.—Calicles widely separated. They are from 1·5 to 2 mm. in diameter, and have from 24 to 28 septal processes. (Plate 2, figs. 2–4.)

Distribution.—Turonian. Gosau, Wolfgangsee, and in Bouches-du-Rhône.

"*Angular Cæca.*"—von Reuss [**16**, Plate 24. fig. 3] has shown some angular cæca, which Lindström has referred to as stellate, and different from anything in *Heliopora*. A very similar arrangement occurs in patches in *Heliopora somaliensis* (*cf.* Plate 2, fig. 8*b*), where it is, however, clearly due to the inclusion of quartz-grains in the coral. The quartz-grains are scattered in patches, which sometimes (as in fig. 8*b*) cut abruptly across a calicle. In some cases these patches were clearly post-mortem in reference to the adjacent calicles, but were formed during the life of the corallum.

Species 3.—POLYTREMACIS MACROSTOMA, von Reuss, 1854.

Synonymy,—Polytremacis macrostoma, von Reuss, 1854. "Kreid Ostalp.," 'Denk. Akad. Wiss. Wien.,' vol. 7, p. 132, Plate 24, figs. 8–10.
„ „ Sardeson, 1896. "Bezieh. Tabul.," 'N. Jahrb.,' Beil. Bd. 10, p. 261.
Heliopora „ Milne Edwards and Haime, 1860. 'Hist. nat. Cor.,' vol. 3, p. 232.
„ *blainvilliana*, *pars*, Quenstedt, 1880. 'Petref. Deut.,' vol. 6, Part 11, p. 901, Plate 178, fig. 30*x*.

Characters.—Calicles 3 to 4 mm. in diameter, and surrounded by about 32 septal ridges. Calicles often widely spaced. (Plate 2, fig. 1.)

Distribution.—Turonian. Gosau.

Species 4.—POLYTREMACIS SEPTIFERA, n.sp.

Synonymy,—Polytremacis blainvilleana, von Reuss, 1854. "Kreid. Ostalp.," 'Denk. Akad. Wiss. Wien,' vol. 7, p. 131, Plate 24, figs. 4–7.

2 A 2

Synonymy,—Polytremacis blainvilliana, *pars*, Milne Edwards an Haime, 1860. 'Hist. nat. Cor. vol. 3, p. 232.

Characters.—Calicles well spaced and provided with from 8 to 2 (usually 8–14) long, well-developed septa. (Plate 2, figs. 5*a*–*b*.)

Distribution.—Turonian. Gosau.

Affinities.—This species is the most Heliolitoid member of the genu

Species 5.—POLYTREMACIS BELLARDI, Haime, 1852.

Synonymy—

Polytremacis bellardi, Haime 1852. "Foss. Numm. Nice," 'Mén Soc. géol. France,' Ser. 2, vol. 4, p. 28 Plate 22, fig. 7.

" " Milne Edwards and Haime, 1860. 'His nat. Cor.,' vol. 3, p. 233.

" " d'Achiardi, 1868. 'Stud. Comp.,' pp. 30, 4

" " " 1875. "Cor. eoc. Friuli," Part ; 'Atti Soc. tosc. Sci. nat vol. 1, p. 206.

Heliopora " von Reuss, 1874. 'Pal. Stud. ält. Tert. Alp Part 3; 'Denk. Ak. Wiss. Wien,' vol. 2 p. 18, Plate 51, figs. 2, 3.

" " Oppenheim, 1896. "Eocaenf. M. Postale 'Palæontogr.,' vol. 43, p. 143.

Millepora globularis, Catullo, 1856. 'Terr. sedimento sup. Venez p. 78, Plate 17, fig. 9.

Heliopora " d'Achiardi, 1867. 'Cat. foss. terr. numm Alpi Venete,' p. 11.

Characters.—Corallum, massive, lobed. Calicles very irregularly d tributed. Septa from 16 to 20, and either short and reduced to sept ridges (*fide* von Reuss), or long, well-developed septa (*fide* Haime).

Distribution.—Eocene. N. Italy.

Genus OCTOTREMACIS, nov.

Synonym—

Polysolenia (*non* Ehrenberg, 1860), von Reuss, 1866. "Foss. K Java": 'Novara Reise, Geol. Th.,' vol. 2, Part 2, p. 172.

Characters.—Helioporidæ with large, well-developed septa, typical eight in each calicle; the septa appear to occur in two cycles.

Type Species.—*Polysolenia hochstetteri*, von Reuss, 1866, *op. cit.*, p. 17 Plate 2, fig. 3.

Distribution.—Miocene. Tjukang Raon, Java.

The type specimen seems to have undergone a double change in fossilisation, and the material injected into the cavities of the coral has been represented by von Reuss as the actual skeleton.

Miscellaneous Indeterminable Species.

(?) *Heliopora mammillosa*, *Millepora mammillosa*, d'Achiardi, 1867. 'Catal. foss. terr. numm. Alpi Venete,' p. 11; von Reuss, 1869. "Pal. Stud. Alt. Tert. Alp.," Part 2; 'Denk. Akad. Wiss. Wien,' vol. 29, p. 352, Plate 27, figs. 4, 5.

Polytremacis bulbosa, d'Orbigny, 1850. 'Prod. Pal.,' vol. 2, p. 183.
,, *complanata* ,, ,, *op. cit.*, p. 209.
,, *glomerata* ,, ,, ,, p. 209.
,, *micropora* ,, ,, ,, p. 209.
,, *provencialis* (,,) ,, ,, p. 209.
,, *ramosa* (,,) ,, ,, p. 183.
,, *subramosa* (,,) ,, ,, p. 209.
,, *supracretacea*, d'Orbigny, 1850. "Foss. Danien." 'Bull. Soc. géol. France,' Ser. 2, vol. 7, p. 134.

REFERENCES IN SECTIONS 1–4.

1. F. Bernard. 'Éléments de Paléontologie,' 1893–1894.
2. H. M. D. de Blainville. 'Manuel d'Actinologie,' 1834.
3. G. C. Bourne, "On the Structure and Affinities of *Heliopora cærulea*, Pallas." 'Phil. Trans.,' B, vol. 186, (1895), pp. 455–483, Plates 10–13.
4. J. D. Dana. "Zoophytes," 'United States Exploring Expedition. Wilkes.' Vol. 8, 1848.
5. Milne Edwards and Haime. 'Histoire naturelle des Coralliaires,' vol. 3, 1860.
6. F. Frech. "Lethaea Geognostica," vol. 1, part 3, 1897.
7. J. W. Gregory. "*Millestroma*, a Cretaceous Milleporoid Coral from Egypt," 'Geol. Mag.,' n.s., dec. 4, vol. 5, 1898, pp. 337–342, Plate 13.
8. G. J. Hinde. Review of Lindström's 'Silurian Corals from China,' 'Geol. Mag.,' n.s., dec. 2, vol. 10, 1883, pp. 86–88.
9. G. Lindström. "Remarks on the Heliolitidæ," 'Handl. k. Svensk. Vet.-Akad.,' vol. 32, No. 1, 1899, 140 pp., 12 Plates.
10. H. Michelin. 'Iconographie Zoophytologique,' 1840–1847.
11. M. Neumayr. 'Die Stämme des Thierreiches,' 1889.
12. H. A. Nicholson. 'Manual of Palæontology,' vol. 1, 1889.
13. H. N. Moseley. "The structure and relations of the Alcyonarian *Heliopora cærulea*, with some account of the Anatomy of a species of *Sarcophyton*, &c.," 'Phil. Trans.,' vol. 166 (1876), pp. 91–130, 2 Plates.
13.* H. N. Moseley. "On Helioporidæ and their Allies," '"Challenger" Reports,' Zool., vol. 2, 1881, pp. 102–126, Plates 1, 2.
14. *A. D. d'Orbigny*. "Note sur des Polypiers fossiles," 1849.
15. ,, "Prodrome de Paléontologie," vol. 2, 1850.

16. A. E. von Reuss. "Beiträge zur Charakteristik der Kreideschichten in de Ostalpen, besonders im Gosauthale und am Wolfgangsee," 'Denk. Aka Wiss. Wien.,' vol. 7, 1854, pp. 1–156, Plates 1–31.
17. F. W. Sardeson. "Ueber die Beziehungen der fossilen Tabulaten zu den Alcy narien," 'N. Jahrb. für Mineral.' Beilage Band 10, 1896, pp. 249–362.
18. Th. Studer. "Versuch eines Systemes der Alcyonaria," 'Arch. Naturgesch. 1887, pp. 1–74, Plate 1.
19. J. Wentzel. "Zur Kenntniss der Zoantharia Tabulata," 'Denk. Akad. Wis Wien.,' vol. 62, 1895, pp. 479–516, 5 Plates.
20. K. A. von Zittel. 'Handbuch der Palæontologie,' vol. 1, Part 2, 1879.

EXPLANATION OF PLATE 2.

Fig. 1. *Polytremacis macrostoma*, Reuss. Turonian. Gosau. B.M., 5582 Part of the external surface of a corallum, showing two calicles partl obliterated by cœnenchymal gemmation, and also radial lines of extern granules, × 3 diam.

Fig. 2. *Polytremacis partschi*, Reuss. Turonian. Russbach, near Gosau. B.M 56,820. Part of transverse horizontal section showing a thick-walle calicle, and the boring of a Leucodorite; × 10 diam.

[As the geological history of the Polychæta is necessarily very impe fect, it is interesting to note the commensalism of a fossil worm wit *Polytremacis* analogous to that of *Leucodora* with *Heliopora cærulea* as no anatomical comparison between the recent and fossil worms possible, it is advisable to refer to the latter simply as Leucodorites.]

Fig. 3. *Polytremacis partschi*, Reuss. Turonian. Gosau. B.M., R4149. Tran verse section across an internal calicle in the *Heliopora* stage, × 10 diam

Fig. 4*a*-*b*. *Polytremacis partschi*, Reuss. Turonian. Bouches-du-Rhône. B.M R2788.

„ 4*a*. Transverse section showing thick calicular wall and radial "costal arrangement of lamellæ. × 10 diam.

„ 4*b*. A transverse section across a primitive cæcal group. × 15 diam.

Fig. 5*a*–*b*. *Polytremacis septifera*, n.sp.; copied from Reuss (16), Plate 24, fig 4 and 5.

„ 5*a*. Portion of a corallum, natural size.

„ 5*b*. Portion of the outer surface, enlarged.

Fig. 6*a*–*d*. *Heliopora cærulea* (Pall.). Recent. B.M., Zool. Dept. Four parts o a growing edge of a lamellar corallum, surface views. × 10 diam.

„ 6*a*. A group of cæca, all subequal in size.

„ 6*b*. A group with enlarged central cæcum.

„ 6*c*. A young calicle and zone of reduced cæca.

„ 6*d*. An older calicle with septal ridges.

Fig. 7*a*–*g*. *Heliolites porosus* (Goldf.). Devonian. Seven stages in the develo ment of a calicle by the fusion of a group of cæca. (After Lindström × 10 diam.

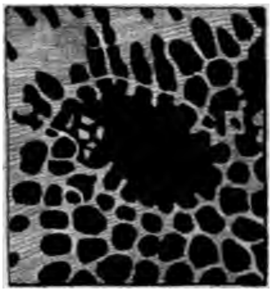
„ 7*a*. A group of cæca.

„ 7*b*. The same slightly developed.

„ 7*c*. The same after the cæcal walls have become thinner by partial absor tion.

„ 7*d*. The same after absorption of the walls of the central cæcum forming calicle made from eight cæca; the cæcal walls remain as septa.

Gregory.

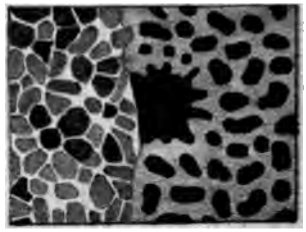


3 ×10

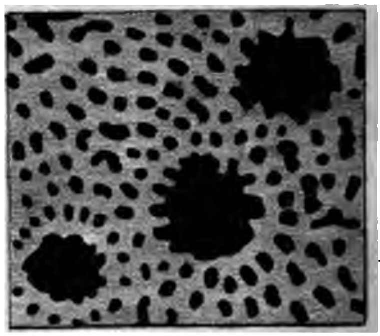
2 ×10

1 ×3

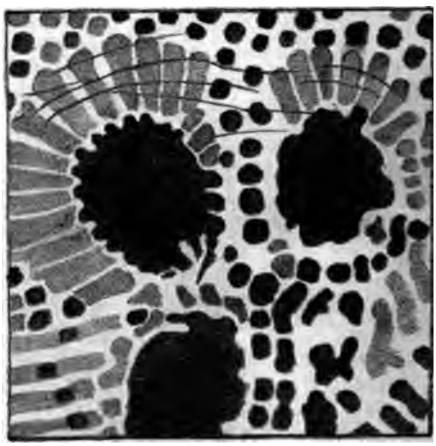
F. Drake del. et lith.



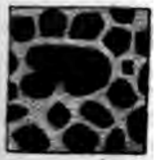
8b ×10



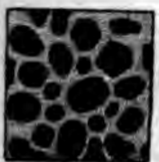
8c ×15



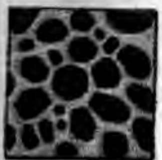
4a ×10



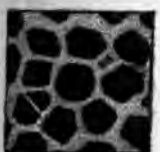
6d ×10



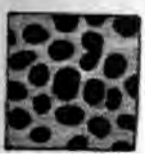
6c ×10



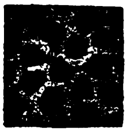
6b ×10



6a ×10



4b ×15

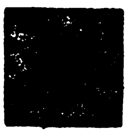


7a ×10

7b ×10

7c ×10

7d ×10

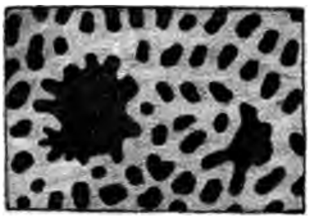


7e ×10

7f ×10

5b

5a



8a ×15

7g ×10

West, Newman imp.

„ *7e.* The same still further developed and after the incorporation of some outer cæca.

„ *7f.* A section across the complete calicle with its twelve well-developed septa.

„ *7g.* The calicle as seen from the surface of the corallum.

Fig. *8a–c.* *Heliopora somaliensis*, n.sp. Turonian. Uradu, Somaliland. B.M., R4150.

„ *8a.* Part of a horizontal section with two calicles. × 15 diam.

„ *8b.* Part of a horizontal section showing circular and angular "cæca," the latter being in the upper portion, which is filled with quartz-grains. × 10 diam.

„ *8c.* Part of a horizontal section, showing three calicles, with and without septal ridges. × 15 diam.

On the Structure of Coccospheres and the Origin of Coccoliths." By Henry H. Dixon, Sc.D., Assistant to the Professor of Botany, Trinity College, Dublin. Communicated by J. Joly, F.R.S., Professor of Geology, Trinity College, Dublin. Received February 3,—Read February 22, 1900.

[Plate 3].

At the beginning of September last year, I visited Valencia, Co. Kerry. It occurred to me there that coccospheres might possibly be drifted in on the warm current of the Gulf Stream, which impinges on the south-west coast of Ireland, and as they float in would become entangled in the sea-weeds on the coast. With this idea, I gathered some of the finer marine algæ, such as species of Cladophora, Polysiphonia, and Plocamium, &c., from the rock pools in Valencia Harbour. Taking care to wash as little of the silt or sediment as possible from them, I fixed the mass in dilute formalin.

This method proved to be a most satisfactory way of collecting coccospheres and coccoliths. In the first sample of sea-weeds thus gathered at a venture, I obtained several hundreds of coccospheres, and of course innumerable coccoliths. In practice, the most convenient way of gathering coccospheres in abundance was found to be to collect the sea-weed, and there and then to wash the sediment from it in sea-water and formalin, or in alcohol, or in sea-water and osmic acid. The sediment which settles down in the fixing fluid will afterwards be found to contain large numbers of coccospheres. In preparations made from material collected in this manner, and mounted under a cover-glass 22 mm. × 22 mm., I have counted as many as fourteen coccospheres. Of course there are many other organisms present in addition to the coccospheres, *e.g.*, various crustacea, mites, worms, molluscs, foraminifera, infusoria, diatoms, peridineæ, &c.

I am indebted for most of the material from which the following

observations are made to Miss Delap and Mr. C. Green, who kindly gathered the algæ, the former at Valencia and the latter at Waterville, and posted them to me, to be then fixed in one or other of the fixing fluids mentioned above.

All the coccospheres found agree in their characters with Wallich's *Coccosphæra pelagica* and all the free coccoliths observed are apparently derived from that organism, all being oval and often possessing the characteristic D-shaped apertures. Throughout my search I did not find a specimen of *Coccosphæra leptopora* of Murray and Blackman, so that apparently this latter is absent from or very scarce in the ocean near our coasts.

The examination of fresh coccospheres in sea-water afforded no evidence as to whether these organisms were alive or dead when collected; no protoplasmic protrusions could be seen extending from the apertures in the coccoliths nor from the spaces between the coccoliths; nor could any spontaneous motion be observed in the coccospheres. In sea-water the coccospheres appear quite colourless, except for the bluish-green appearance of the covering coccoliths, due apparently to the fact that their refractive index is higher than the water in which they are immersed. No coloration due to the presence of an internal chromatophore nor any sign of such a body could in any case be made out.

The absence of the chromatophore, recorded by Murray and Blackman as occurring in *C. leptopora*, is not, I think, sufficient reason for concluding that all the coccospheres examined had been for some time dead, and so had lost their cell contents; for the coccospheres were in the great majority of cases quite perfect, and, as we will see later on, the presence of proteid material and probably of a nucleus in a large number of them was revealed by various stains and reagents. Some, however, were devoid of contents or possessed so little that their presence eluded detection.

External Characters.

As Murray and Blackman* point out, there is considerable variation in the size of *Coccosphæra pelagica*. Of about fifty specimens taken at random and measured, the largest had a diameter measuring 0·0294 mm., and the smallest 0·0199 mm. This variation is in part due to the inconstancy in the number of coccoliths on the coccospheres, and in part to the varying amount of overlap of the coccoliths over one another. The greatest number of coccoliths observed by me on one coccosphere was sixteen and the smallest six. Fig. 1 (Plate 3) shows a coccosphere captured in 1897 in Killiney Bay, having only six or seven coccoliths upon it. Fig. 2 shows a fragment of another coccosphere

* 'Phil. Trans.,' B, vol. 190 (1898), pp. 427—441.

equally small, taken in Valencia Harbour in 1899. It is difficult to estimate with certainity the number of coccoliths on the coccosphere while it is intact, and one is apt to under-estimate it. But it is possible to make sure of accuracy by crushing coccospheres mounted in glycerine jelly. The coccoliths are then sufficiently separated from one another to enable one to count them easily, while the viscidity of the mounting medium does not allow any of them to be lost. It was from coccospheres treated in this manner that the maximum and minimum numbers here stated were derived.

The variation in the amount of overlap brings it to pass that sometimes coccospheres with a small number of coccoliths upon them have an equal or even greater diameter than others which carry a larger number of coccoliths. To quote an example :—Two coccospheres were found, each measuring 0·021 mm. in diameter; one of these carried ten coccoliths and the other only seven.

The coccoliths, although they do not vary so much as the coccospheres in their dimensions, yet differ from one another considerably in size. From a large number of measurements of the long axis of the oval, it was found that the extremes deviated from the mean by as much as 20 per cent., the maximum being 0·018 mm. and the minimum 0·013 mm., while the mean was 0·015 mm.

This variation in size of the component coccoliths is in part but not, I think, to a large extent, responsible for the variation in the size of the coccospheres, as it is found that coccoliths of different sizes occur on one and the same coccosphere. It will be seen later that there is some reason to believe that the smaller coccoliths are formed in the early stages of the coccosphere's life history when it is itself small. It may be noted that the smaller coccoliths on a coccosphere are often, if not usually, without the transverse bar across the central perforation.

Some of the finer details of the structure of the coccoliths are more easily made out when these latter are examined *in situ* on the coccospheres, as then in the one field coccoliths may be examined in almost every position, and views from different directions may be carefully compared. Broken coccospheres, too, often afford valuable evidence as to the relations of the coccoliths to one another and their interlocking. This last point, as well as the shape of the body surrounded by the valves, is greatly elucidated by observations made on microtome sections of material embedded in paraffin. The most instructive preparations were obtained from sections 5μ—10μ thick; often those of 10μ are more satisfactory than the thinner ones, as, despite the support afforded by the paraffin, the coccoliths in the latter are so often badly shaken and cracked that the fine details of their structure are not so well seen as they are to the thicker sections.

When a coccolith is examined in actual or optical vertical section, it is

seen that the oval disc, which I shall call the body of the coccolith, forming the bottom of the central depression and carrying the single slit or two D-shaped holes, is of some considerable thickness (figs. 3, 4, and 5). Its inner surface projects very slightly inside the inner valve, and the slit or D-shaped perforations are enlarged towards its inner and outer surfaces. The whole coccolith consequently has the form of a very short, thick-walled, oval tube, with its outer extremity recurved to form the outer valve. The inner valve is a dished collar attached very close to the inner extremity of the tube. The projection of the tube-like portion, or body, inside the inner valve is very slight, and can only be made out either by sections or by very careful focussing.

An examination of the coccoliths in plan has also revealed a few points of interest. Where the body of the coccolith comes in contact with the depressed portion of the outer valve, there are to be seen a series of very minute punctations (fig. 6) corresponding, as far as could be ascertained, with the radiating grooves between the striations of the valve. It is possible that these punctations represent the ends of minute passages running down the outside of the body and inside the valves of the coccolith. Another point that may be noticed has reference to the material composing the transverse bar usually present, dividing the slit-like canal of the body into the two D-shaped holes. This bar is often obviously composed of somewhat different material from the rest of the coccolith, and this difference extends for some distance beyond the edges of the slit continuing the direction of the bar. In appearance the bar and its continuation are less highly refractive than the rest; and the difference of material is further manifested by the fact that it dissolves more rapidly in weak acids than the body of the coccolith, while this latter goes into solution more readily than the collar-shaped portion uniting the two valves. It must be borne in mind, however, that this difference in the rates of solution may be in part or completely due to a difference in the ease of diffusion round these parts. It will be seen later that the order of solution is the reverse of the order of development. The oldest parts of the coccolith are the last to be dissolved.

In a previous note Dr. Joly* and I have already noticed that there is no evolution of free gas when coccoliths are attacked by acids. This is probably due to the fact that the amount of gas they generate is unable to overcome the cohesion of the liquid in which the reaction takes place. From this consideration it would appear that the absence of free gas under the action of acids is no objection to regarding coccoliths as composed in the main of calcium carbonate. To test the validity of this view I mounted some fine precipitated calcium carbonate in water, which had been boiled so that it thoroughly wetted the

* 'Nature,' September 16, 1897.

crystalline precipitate. While this preparation was under observation, dilute acid was introduced under the cover-glass, and it was found that gas bubbles were generated in connection with the larger crystalline aggregates only, while the smaller ones dissolved without any gas appearing in their immediate neighbourhood, even after the liquid was completely saturated, and contained much free gas. Crystalline masses having a diameter of 0·027 mm. dissolved in this manner without the formation of bubbles. In the case of the solution of the larger masses, an increase in size of the bubbles previously existing in their neighbourhood was observable. It seems quite probable that if greater care were taken to eliminate all free gas from the water in which the crystals were mounted and from the added acid, that much larger masses of calcium carbonate might be brought into solution without the evolution of bubbles, as then the cohesion of the liquid would have to be overcome before they could appear. To overcome this would require a force of many atmospheres. In any case the observation quoted shows that solid masses of calcium carbonate having the same diameter as a coccosphere (and consequently containing a great deal more calcium carbonate), dissolve without producing free carbon dioxide.

Besides their ready solution in weak acids, the behaviour of coccoliths towards picric nigrosine is very characteristic of calcium carbonate. While the coccolith is dissolving in this stain a dense, dark, but extremely fine precipitate or coloration is formed on its surface. The same reaction may be observed in the solution of small crystals of calcium carbonate. In the case of the coccolith the precipitate is most marked in the D-shaped holes, round the collar connecting the two valves, and along the radial striæ on the valves. It seems probable that the remaining parts of the valves do not possess sufficient material to render the reaction apparent in their case. Besides thus affording additional evidence as to the nature of the material forming coccoliths, this reaction makes several obscure points in their structure stand out with great clearness.

That the coccolith is not pure calcium carbonate appears from its behaviour towards a 1 per cent. solution of sodium carbonate. When mounted in this reagent the coccolith after a short time assumes a peculiar appearance. Its surface becomes lumpy and loses its clean-cut contour. Some coccoliths exhibit this change much more markedly than others, and all more plainly towards their periphery than at the centre. It is probable that the change is brought about by the solution of the organic basis remaining over in the coccolith. The completeness of the replacement of the organic basis by calcium carbonate would then determine the extent of the change.* The same change is

* Calcareous sponge spicules undergo a similar change when treated with this reagent.

noticeable, but to a much less degree, when coccoliths are treated with a 20 per cent. solution of sodium chloride.

On the coccosphere the coccoliths overlap each other to a considerable extent. The amount of overlap is not constant. It is usual for the outer valve of one to penetrate between the outer and inner valves of its neighbour, so far as to reach the collar uniting the two valves. (Figs. 7, 8, 9 and 4.) In this way the coccoliths on the surface of a coccosphere interlock with one another, and form a comparatively rigid shell. The bevelled shape of the coccolith is evidently necessitated by the overlapping on a curved surface. The rigidity of this form of structure is best appreciated from the examination of fragments of a broken coccosphere, which will preserve their curvature even when they are composed of only three or four coccoliths. Such a fragment is shown in fig. 2. Indeed the interlocking of the coccoliths is so complete that it seems impossible to break up a coccosphere without at the same time splitting the valves of several of its coccoliths. Some distortion in its shape is, however, possible without fracturing the coccoliths. This yield is possibly due to the give in the outer region of the valves, which appear to contain a considerable amount of organic material in their composition.

If coccospheres are dissolved in acid and subsequently or simultaneously stained, appearances are observed which seem to point to the existence of an extremely fine pellicle covering over the coccoliths and enclosing the whole sphere. The simplest method of demonstrating this pellicle is by mixing up in glycerine jelly a stain to which a trace of nitric or hydrochloric acid has been added. The coccospheres are then mounted in this medium, and the solution and staining go on simultaneously. Or again the material containing the coccospheres may be mounted in feebly acidulated jelly, and then a drop of the stain selected applied to the edge of the cover-glass. Either of these methods give good results (fig. 10) if the solution is sufficiently slow. The best results are obtained when the coccoliths take a fortnight or more to disappear. The stains I used were acid fuchsine and methyl green, fuchsine and iodine green, aniline blue, aniline green, and nigrosine.

After the solution of the coccoliths and staining it is seen (fig. 10) that the coccosphere is bounded by a pellicle of extreme tenuity. In the pellicle are a number of oval holes, corresponding in position and number to the central depressions of the coccoliths which have disappeared. The edges of the oval perforations are jagged, and from the irregular teeth extend radial striæ, which perhaps correspond to the striæ of the coccoliths. The striæ from two adjacent perforations are often continuous. The jagged teeth of the pellicle may often be made out by careful focussing of the surface of the coccosphere, even before it is attacked by acids and stained. Sometimes in dissolved

coccospheres there is a residue of stained material left in the central part of the holes, corresponding roughly in shape and position to the D-shaped perforations of the body of the coccoliths. This residue may possibly be the remains of protoplasmic filaments or protrusions once extending from the central region of the coccosphere. Similar granular slimy masses may frequently be seen in stained* and undissolved coccospheres occupying more or less of the central depressions of the coccoliths; but their irregularity of occurrence and shape make it impossible to say whether they are proteid material in connection with and derived from the central body of the coccosphere, or slime adventitiously deposited from the surrounding liquid.

Internal Structure.

By continuing the solution with acid, several points of the internal structure of coccospheres become apparent. The external pellicle gradually disappears, and only a number of disconnected, very minute granules persist to mark its position. The jagged edges of the holes persist the longest, but finally they also dissolve. If the coccosphere is stained in this condition it will appear that immediately within the external pellicle which has now disappeared there is a slimy proteid material, in which the inner valves and bodies of the covering coccoliths were embedded. Sometimes after treatment with nitric acid and staining with aniline blue this material has a homogeneous appearance, and is uniformly stained, while the positions once occupied by the coccoliths are colourless and transparent. More frequently, however, it is a finely and sparsely granular slime (fig. 9). It gives a faint orange coloration with nitric acid, followed by ammonia.

This slimy layer, in which the coccoliths are embedded, is bounded internally by a gelatinous, transparent, and sometimes slightly stratified membrane (figs. 11—14). There is usually no definite demarcation between the two, but the membrane passes imperceptibly into the slimy material outside. The internal valves of the coccoliths rest upon this membrame, and it is often seen to be drawn out into prominences corresponding with the position of the coccoliths on its outside. In optical section discontinuities are sometimes apparent in this membrane (figs. 13, 14), and it may be that these represent perforations corresponding with the perforations of the coccoliths, and through which the internal protoplasm communicates with the external surroundings. The membrane itself is difficult to stain, and it, like the

* I have found Bismarck brown, aniline blue, acid fuchsine, and methyl green useful stains for bringing out the structure of undissolved coccospheres. Delafield's hæmatoxylin sometimes gives very good results when precipitations do not occur. Material stained with the last mentioned is most satisfactory for microtome sectioning.

slimy layer, exhibits a slight orange coloration when treated with nitric acid followed by ammonia. With Schultze's solution it becomes amber-coloured.

Within this internal membrane in all my specimens but scanty protoplasmic contents were revealed, but in these there was often distinguishable a minute, more darkly staining body, presumably a nucleus. In several specimens this nucleus was double or hour-glass shaped (figs. 12 and 15). My specimens did not show the structure of the nucleus with precision, and nothing beyond its granular appearance and probable possession of a membrane could be made out. The aggregation of protoplasm in which the nucleus is situated occupies a lateral position, and is in contact with the internal membrane, and strands of protoplasm extend from it across the cavity of the sphere. In no case was a chromatophore, nor anything like one, seen in the protoplasm. In one case a colourless trilobed body was found in the cell; its nature is quite uncertain. Fig. 16 is drawn of it after its containing coccosphere was treated with *liquor iodi.* The unfavourable conditions of our coasts are, perhaps, responsible for the scantiness of the protoplasmic contents of the coccospheres obtained in the south of Ireland. But to these unfavourable conditions can scarcely be attributed the absence of the chromatophore, for the existence of which in *C. pelagica* we have no definite evidence.

I now go on to a series of observations which seem to me to be of considerable interest, as throwing some light on the manner of growth of coccospheres, and on the origin and development of coccoliths. If entire coccospheres are examined in a medium of high refractive index, *e.g.*, balsam, or even glycerine jelly, it will be found that numerically about 80 per cent. of them contain an internal oval colourless body. Closer examination reveals that this body is in many cases a complete and perfect coccolith (fig. 17), in others it is a simple oval ring or shallow collar (figs. 3, 7, 8, 15). All stages of development connecting the collar-shaped body with the complete coccolith are found, so that it becomes evident that the coccolith arises as a ring of calcium carbonate within the coccosphere. At first the ring is a narrow band (fig. 3), it then deepens into a collar (fig. 8); on either end of the collar are then secreted oblique flanges (fig. 15), which, as deposition continues, are developed into the bevelled valves of the coccolith (figs. 7 and 17). The central body appears last, and only in its later stages is the transverse bar secreted, dividing its single aperture into two. The position of the internal coccolith within the coccosphere varies; generally speaking, in its earlier stages of development, it lies near the centre of the sphere, and in many cases it was found in contact with the nucleus (figs. 15, 17). When more mature it comes into contact with the inner gelatinous membrane, and when the coccosphere is intact, appears in close proximity to the external coccoliths

(fig. 18). It is presumably to be inferred that it is finally extruded between these latter, and takes up its position in the shell of the sphere.

The internal coccolith gives the same reactions as the outer ones. When a coccosphere is acted on by acids, the internal coccolith, if present, is the last (as we might expect) to dissolve (fig. 14), and in its solution its radial striæ are the last parts of it to disappear. Under treatment with picric-nigrosine, the behaviour of a coccosphere containing an internal coccolith is characteristic. First the external coccoliths darken, and their striæ and other details stand out with great clearness; as solution proceeds, a very dark coloration covers all the outer coccoliths, and when this clears away, their solution is all but complete. The internal coccolith then goes through the same phases as the outer ones; its striæ become clearer, it darkens, and, as it goes into solution, the whole cavity of the sphere becomes filled with a dark green coloration. The appearance of this coloration is sometimes very sudden; it disappears with less rapidity. When it is gone, the scanty protoplasm, nucleus, gelatinous and slimy envelopes stained with the nigrosine are all that are left of the coccosphere.

As a general rule, only one internal coccolith can be made out in each coccosphere; sometimes, however, one mature coccolith and one in a very early stage are found. In the coccosphere (fig. 16), which contained the central trilobed body alluded to above, so far as I could ascertain a ring-shaped coccolith was in contact with each lobe. But this observation is open to doubt, as the coccosphere was mounted in water, so that the internal coccoliths were only indistinctly seen, and unfortunately the solution of both internal and external coccoliths took place while they were not under observation.

In the case of internal coccoliths which have almost reached maturity, it is generally possible to perceive that they are somewhat larger than any of the coccoliths already in position on the sphere. It would appear from this that the coccoliths formed by a coccosphere in its earlier stages are smaller than those developed later in its life history. Indeed, measurements of coccoliths and coccospheres almost necessitate this conclusion. Thus coccoliths are often found with their longer diameter equal to 0·018 mm., while the internal measurements of some of the smaller coccospheres could not accommodate a coccolith of these dimensions. Again, it is found that the coccoliths with a simple slit-like perforation in their central body are, as a rule, smaller than those with D-shaped perforations; so that we may with some probability assume that the single perforation is the more primitive condition, and that coccoliths with it only are formed in the coccosphere during its earlier stages. The history of the development of the coccolith also points in this direction.

It appears that the extrusion of a coccolith to the surface must

lead to a readjustment of the coccoliths already in position. In ord to effect this, they must slide past each other, and take up such re tive positions with one another and the new coccolith as will chan the curvature of the surface and increase the size of the sphere. T dished shape of the coccoliths and their oval outline are apparent adaptations to accommodate this peculiar method of growth. T oval form and the method of intercalating new plates in the skeletc necessitates the overlap; but this expenditure of material is co pensated by the great rigidity so obtained, and the complete protecti of the organism.

It is evident that these observations on the internal origin ai development of coccoliths dispose of those theories which regard t coccospheres either as reproductive bodies of the coccoliths or as inc pendent organisms which aggregate coccoliths on their surface, as sor Rhizopoda gather diatom skeletons. The observations also render i probable the view that the coccoliths are formed as precipitates dead organic slimes, as has been suggested. The history of the develc ment shows that the coccosphere is the organism which secretes t coccoliths as its skeleton, and it is probable that these latter only occ free in the water after the death and disintegration of the pare coccosphere.

Summary.

The following conclusions may be drawn from the foregoing obs vations:—

1. The body of the coccolith extends for a short distance inside t internal valve.

2. The coccolith is composed of calcium carbonate and a trace some substance soluble in 1 per cent. of sodium carbonate.

3. Coccospheres are covered over with an extremely delicate p licle, which is less readily soluble in dilute acids than the coccolit within it.

4. The coccoliths on a coccosphere are partially embedded in a slin proteid material.

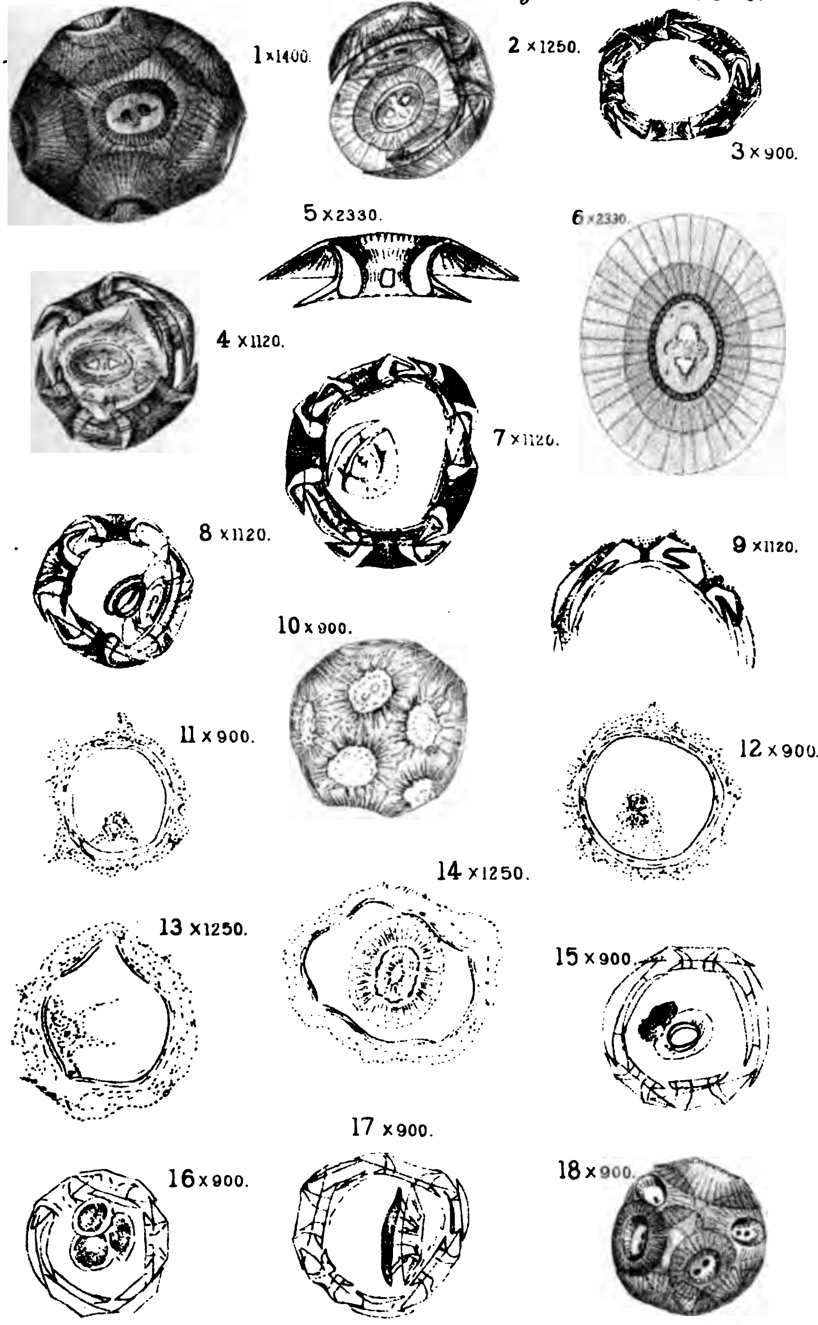
5. Within the slimy layer there is a somewhat stratified intern spherical membrane.

6. The specimens of coccospheres examined contained no chromat phore.

7. In many instances the presence of a minute internal body, pr sumably a nucleus, was demonstrated.

8. Coccoliths are secreted internally in close proximity to tl nucleus; the collar uniting the valves is first formed, then the valv are developed, and finally the central body of the coccolith is secrete

9. The coccolith, when complete, is probably extruded to the su face, and takes up its position among its predecessors. Its valv


1 ×1400.
2 ×1250.
3 ×900.
5 ×2330.
6 ×2330.
4 ×1120.
7 ×1120.
8 ×1120.
9 ×1120.
10 ×900.
11 ×900.
12 ×900.
14 ×1250.
13 ×1250.
15 ×900.
17 ×900.
16 ×900.
18 ×900.

A. S. Huth, Lith.

become interlocked with those of its neighbours. By this intercalation an increase of the volume of the sphere is provided for.

10. The oval and dished form of the coccoliths are adaptations to allow of the rearrangement of the older coccoliths on the extrusion of a new one, and of suitable interlocking on the spherical surface.

EXPLANATION OF PLATE 3.

All the figures are drawn by means of a *camera lucida* from specimens obtained at Valencia and Waterville, Co. Kerry, except Fig. 1.

Fig. 1.—Coccosphere, × 1400, drawn from a fresh specimen captured September, 1897, in Killiney Bay.

„ 2.—Fragment of small coccosphere composed of four interlocking coccoliths. × 1250.

„ 3.—Microtome section of coccosphere, showing an early stage in the development of an internal coccolith. × 900.

„ 4.—Microtome section of coccosphere, showing coccoliths in section. × 1120.

Figs. 5 and 6.—Single coccolith in section and plan.

Fig. 7.—Microtome section of coccosphere, showing mature internal coccolith. × 1120.

„ 8.—Microtome section of coccosphere, with partially developed internal coccolith. × 1120.

„ 9.—Microtome section of coccosphere after solution of coccoliths and staining with Delafield's hæmatoxylin. × 1120.

„ 10.—Outer pellicle remaining over after the coccoliths of a coccosphere are dissolved in acid glycerine jelly and stained in aniline blue. × 900.

Figs. 11 and 12.—Coccospheres after prolonged action of picric nigrosine, × 900, showing the slimy layer, stratified membrane, and the nucleus within the latter.

Fig. 13.—Same as 12. × 1250.

„ 14.—Coccosphere partially dissolved in picric nigrosine, showing the internal coccolith in early stages of solution.

„ 15.—Optical section of a coccosphere, × 900. Within the external coccoliths are seen the constricted nucleus and a nearly mature coccolith in contact with the nucleus. The specimen is stained with aniline blue soluble in alcohol.

„ 16.—Optical section of a coccosphere after treatment with *liquor iodi*. × 900. Inside the coccoliths is seen a problematical trilobed body.

„ 17.—Optical section of a coccosphere, showing nucleus in contact with a completely developed internal coccolith, stained in aniline blue soluble in water. × 900.

„ 18.—A coccosphere showing an internal coccolith immediately beneath the external coccoliths. × 900.

"Data for the Problem of Evolution in Man. IV. Note on the Effect of Fertility depending on Homogamy." By KARL PEARSON, F.R.S., University College, London. Received March 12,—Read March 29, 1900.

1. In a paper recently contributed to the 'Proceedings of the Royal Society,' "Data for the Problem of Evolution in Man. III. On the Magnitude of Certain Coefficients of Correlation in Man,"* &c., I dealt with the problem of the possible dependence of fertility on homogamy and I used the following words (p. 29):—

"When any form of life breaks up into two groups under the influence of natural selection, what is to prevent them intercrossing and so destroying the differentiation at each fresh reproductive stage?"

The answer I suggested was twofold—(i) homogamy, which I can demonstrate to actually exist in the case of man, and (ii) a possible dependence of fertility on homogamy, which would render the cross unions relatively sterile. Either (i) or (ii) would be effective, but (ii) would have the advantage that it does not presuppose assortative mating; we could have a permanent differentiation even with random mating. In writing the above sentence, I had two further points in mind: (*a*) that reproductive selection, while quite capable of producing an evolution, a progressive change in a species, could not by itself differentiate a species into two sub-groups, and (*b*) that no correlation of homogamy with fertility could possibly differentiate a species, however much it might cause the species to progressively change as a whole. My view was that a correlation of homogamy with fertility together with natural selection, could produce a permanent differentiation of species, but that neither *alone* could be effectual. It was from this standpoint that I concluded my paper with the words:—

"I can conceive no more valuable investigation than a series of experiments or measurements directed to ascertaining whether homogamy is or is not correlated with fertility" (p. 32).

In writing these words I overlooked a very admirable piece of work by Mr. H. M. Vernon, M.A., published in our own 'Transactions,' on "The Relations between the Hybrid and Parent Forms of Echinoid Larvæ."† Had I been acquainted with this memoir, I should certainly have referred to it. In drawing my attention to it, Mr. Vernon has also referred me to two papers by himself in 'Natural Science,' on what he terms "Reproductive Divergence."‡ While welcoming heartily Mr. Vernon's facts in the paper on "Echinoid Larvæ," bearing

* 'Proc. Roy. Soc.,' vol. 66, p. 28.

† 'Phil. Trans.,' B, vol. 190, p. 465—529.

‡ Vol. 11, pp. 181—189 and pp. 404—407.

on the correlation of fertility and homogamy, I want at once to express my entire disagreement with his view of reproductive selection, if he holds it, as he appears to do,* as a source of divergence or differentiation *quite independent of natural selection.*

The simple fact is, that if fertility be any function of the organs or relative organs of the parents, having a frequency distribution defined by a normal frequency surface, or by any surface approximating to such a chance distribution, then reproductive selection, whether homogamy or any other factor be present, may, under special circumstances, produce a progressive change in a character; it cannot, unless other factors of evolution, such as natural selection, come into play, produce differentiation.

2. I shall assume throughout my proof that the frequency distributions obey the normal law. Now let one offspring only be taken from each pair of parents, and let the organs in the two parents be $m_1 + x_1$, $m_2 + x_2$, and in the offspring $m_3 + x_3$, where m_1, m_2, m_3, are the respective means; let σ_1, σ_2, σ_3 be the standard deviations of the three organs, and r_{12}, r_{23}, r_{31}, the coefficients of correlation, then the frequency $z\delta x_1\, \delta x_2\, \delta x_3$ of a triplet of parents and offspring with organs lying between x_1 and $x_1 + \delta x_1$, $x_2 + \delta x_2$, x_3 and $x_3 + \delta x_3$, respectively, is determined by†

$$z = \frac{\mathrm{N}}{(2\pi)^{\frac{3}{2}}\sigma_1\sigma_2\sigma_3\sqrt{\chi}} \text{ expt. } \Bigg[-\frac{1}{2\chi}\Bigg\{\frac{x_1^2}{\sigma_1^2}(1-r_{23}^2)+\frac{x_2^2}{\sigma_2^2}(1-r_{31}^2) + \frac{x_3^2}{\sigma_3^2}(1-r_{12}^2) - 2(r_{12}-r_{23}r_{31})\frac{x_1x_2}{\sigma_1\sigma_2} - 2(r_{23}-r_{31}r_{12})\frac{x_2x_3}{\sigma_2\sigma_3} - 2(r_{31}-r_{12}r_{23})\frac{x_3x_1}{\sigma_3\sigma_1}\Bigg\}\Bigg],$$

where $\chi = 1 - r_{12}^2 - r_{23}^2 - r_{31}^2 + 2r_{12}r_{23}r_{31}$,

and N = total number of pairs of parents.

If, instead of the single offspring, we take n, we have only to replace N in the above results by nN.

Now reproductive selection supposes the fertility of a given pair not to be independent of the measure of their organs, in this case of $m_1 + x_1$ and $m_2 + x_2$.

If we suppose n to represent the total fertility of a given pair, we

* "This divergence of species takes place quite independently of natural selection, but this principle can always be exerting its action at the same time, whereby the new or modified characteristics produced can, if useful to the species, be accumulated and rendered better adapted to the environmental conditions." 'Natural Science,' vol. 11, p. 186.

† 'Phil. Trans.,' A, vol. 187, p. 287.

shall, on the hypothesis of the normal law holding for frequency of offspring, have

$$n = n_0^{-\frac{1}{2}\frac{y^2}{s^2}},$$

where y is the deviation of some character based upon both parental organs from the value which gives the maximum fertility, and s is its standard deviation.

Thus, c and a denoting constants,

$$\begin{aligned} y &= f(m_1+x_1,\ m_2+x_2) \\ &= c_0+c_1x_1+c_2x_2+\text{higher terms in } x_1 \text{ and } x_2; \end{aligned}$$

hence $$y^2 = a_0+a_1x_1+a_2x_2+a_3x_1^2+2a_4x_1x_2+a_5x_2^2,$$

if we neglect higher powers of x_1 and x_2. This will, as a rule, be justified if x_1 and x_2 are small as compared with m_1 and m_2.

We conclude that:

$$n = n_0 \text{ expt.}\left[-\frac{1}{2s^2}(a_0+a_1x_1+a_2x_2+a_3x_1^2+2a_4x_1x_2+a_5x_2^2)\right].$$

If we multiply this by z, we have the distribution of parents and offspring, allowing for a varying fertility. Let this be z', then it will be at once obvious that

$$z' = \text{const.} \times \text{expt.}\ [-(\text{quadratic expression in } x_1, x_2, x_3)].$$

Hence if we integrate for x_1 and x_2, so as to get the distribution of the offspring, we find it again given by a normal curve, *i.e.*, a curve symmetrical about its mode. *Thus a progressive change, but no differentiation, can be produced by reproductive selection.*

This is the proof, of which I merely stated the result in my "Note on Reproductive Selection" of 1895.* The same view was again expressed in my memoir on "Genetic (Reproductive) Selection" of 1898.† While reproductive selection is invaluable as an aid to natural selection, alone it can only progressively modify not differentiate a race. For such differentiation we should have to suppose some much more elaborate relation between fertility and the complex of parental organs than is indicated by a normal chance distribution.

3. In order to show what would occur supposing fertility reached a maximum with homogamous unions, I do not simply take y to be the difference of the parental organs, for it is quite conceivable that the organs may be sexual characters, and differ not only in magnitude but even qualitatively. I accordingly suppose the fertility to be a maximum

* 'Roy. Soc. Proc.,' vol. 59, p. 303.

† 'Phil. Trans.,' A, vol. 192, p. 314.

when the two organs bear a certain ratio to each other. For example, we hardly mean by a homogamous union in man and woman with regard to stature, a case of husband and wife of equal height, but rather a case of their being *relatively* of equal height, or, say, the ratio of their statures = 1·08.*

For this reason I put

$$y = p_1(m_1+x_1) - p_2(m_2+x_2),$$

and asked Mr. L. N. G. Filon, M.A., to work out for me the constants of the correlation surface, whose ordinate is $z' = z \times n$. He has kindly provided me with the following results, the analysis being straightforward but lengthy.

Let $m_1 + h_1$, $m_2 + h_2$ be the mean values of the organs in the parents, each parent being repeated for each of his or her offspring. $m_3 + h_3 =$ mean value of offspring's organ, or h_3 be the progression in the character due to the influence of homogamy.

Σ_1, Σ_2, the standard deviations of the parents' organs, these being, as in the case of h_1 and h_2, weighted with their fertility.

Σ_3 = standard deviation in offspring's organ, or $\Sigma_3 - \sigma_3$ is the change in variability due to the homogamous influence.

ρ_{31}, ρ_{32} = the correlations between parent and offspring when we take *all*, and not a single offspring from each union.

ρ_{12} the coefficient of assortative mating when we take each pair as many times as there are offspring of the union.

We have:

$$\frac{h_1}{\sigma_1} = \frac{h_2}{\sigma_2} = -\frac{(p_1\sigma_1 - p_2\sigma_2)(p_1m_1 - p_2m_2)}{s^2 + (p_1\sigma_1 - p_2\sigma_2)^2} \quad \ldots\ldots\ldots\ldots \quad \text{(i).}$$

$$\frac{h_3}{\sigma_3} = -\frac{r_{31} + r_{32}}{1 + r_{12}} \frac{(p_1\sigma_1 - p_2\sigma_2)(p_1m_1 - p_2m_2)}{s^2 + (p_1\sigma_1 - p_2\sigma_2)^2} \ldots\ldots\ldots\ldots \quad \text{(ii).}$$

This last result may be written

$$\frac{h_3}{\sigma_3} = \frac{r_{31} - r_{12}r_{32}}{1 - r_{12}^2} \frac{h_1}{\sigma_1} + \frac{r_{32} - r_{12}r_{31}}{1 - r_{12}^2} \frac{h_2}{\sigma_2} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad \text{(iii).}$$

$$\Sigma_1^2 = \sigma_1^2\left(1 - \frac{(p_1\sigma_1 - r_{12}p_2\sigma_2)^2}{s^2 + p_1^2\sigma_1^2 + p_2^2\sigma_2^2 - 2r_{12}p_1p_2\sigma_1\sigma_2}\right) \ldots\ldots \quad \text{(iv).}$$

$$\Sigma_2^2 = \sigma_2^2\left(1 - \frac{(r_{12}p_1\sigma_1 - p_2\sigma_2)^2}{s^2 + p_1^2\sigma_1^2 + p_2^2\sigma_2^2 - 2r_{12}p_1p_2\sigma_1\sigma_2}\right) \ldots\ldots\ldots \quad \text{(v).}$$

$$\Sigma_3^2 = \sigma_3^2\left(1 - \frac{(r_{13}p_1\sigma_1 - r_{23}p_2\sigma_2)^2}{s^2 + p_1^2\sigma_1^2 + p_2^2\sigma_2^2 - 2r_{12}p_1p_2\sigma_1\sigma_2}\right) \ldots\ldots \quad \text{(vi).}$$

* This is how I have looked at the matter in "Data for the Problem of Evolution in Man. III," 'Roy. Soc. Proc.,' vol. 66, p. 31.

$$\rho_{12} = \frac{r_{12} + \frac{p_1 p_2 \sigma_1 \sigma_2}{s^2}(1 - r_{12}^2)}{\sqrt{\left\{1 + \frac{p_1^2\sigma_1^2}{s^2}(1 - r_{12}^2)\right\}\left\{1 + \frac{p_2^2\sigma_2^2}{s^2}(1 - r_{12}^2)\right\}}} \quad \ldots\ldots \quad \text{(vii)}.$$

$$\rho_{31} = \frac{r_{31} + \frac{p_1 p_2 \sigma_1 \sigma_2}{s^2}(r_{32} - r_{12} r_{31}) + \frac{p_2^2\sigma_2^2}{s^2}(r_{31} - r_{12} r_{32})}{\sqrt{1 + \left\{\frac{p_1^2\sigma_1^2}{s^2}(1 - r_{31}^2) + \frac{p_2^2\sigma_2^2}{s^2}(1 - r_{32}^2) - \frac{2p_1 p_2 \sigma_1 \sigma_2}{s^2}(r_{12} - r_{31} r_{32})\right\}} \times \sqrt{\left\{1 + \frac{p_2^2\sigma_2^2}{s^2}(1 - r_{12}^2)\right\}}}$$

$$\ldots\ldots\ldots\ldots \quad \text{(viii)}.$$

$$\rho_{32} = \frac{r_{32} + \frac{p_1 p_2 \sigma_1 \sigma_2}{s^2}(r_{31} - r_{12} r_{32}) + \frac{p_1^2\sigma_1^2}{s^2}(r_{32} - r_{12} r_{31})}{\sqrt{\left\{1 + \frac{p_1^2\sigma_1^2}{s^2}(1 - r_{31}^2) + \frac{p_2^2\sigma_2^2}{s^2}(1 - r_{32}^2) - \frac{2p_1 p_2 \sigma_1 \sigma_2}{s^2}(r_{12} - r_{31} r_{32})\right\}} \times \sqrt{\left\{1 + \frac{p_1^2\sigma_1^2}{s^2}(1 - r_{12}^2)\right\}}}$$

$$\ldots\ldots\ldots\ldots \quad \text{(ix)}.$$

Results (i) to (ix) contain the whole theory of the influence on evolution of a relation between homogamy and fertility.

4. *General Conclusions.*—(*a*) There is in general a progressive change in the species as a whole, but no divergence or differentiation.

(*b*) The change in the second generation (as given by (iii)) is precisely what we might have anticipated from my theory of biparental inheritance,* assuming that the offspring are those of parents differing from the general population by an amount of the character which is the excess marking parents weighted by their fertility from the general parental population.

(*c*) The offspring will be less variable than they would be without a correlation between homogamy and fertility, *i.e.*, from (vi) Σ_3 is always less than σ_3.

* 'Roy. Soc. Proc.,' vol. 58, p. 240, or 'Phil. Trans.,' vol. 187, p. 287. Another interesting relation of this kind is the following one:

$$\sigma_3 \sqrt{\frac{1 - r_{12}^2 - r_{23}^2 - r_{31}^2 + 2r_{12}r_{23}r_{31}}{1 - r_{12}^2}} = \Sigma_3 \sqrt{\frac{1 - \rho_{12}^2 - \rho_{23}^2 - \rho_{31}^2 + 2\rho_{12}\rho_{23}\rho_{31}}{1 - \rho_{12}^2}};$$

or the variability of an array of offspring from selected parents is unaltered by the relation between homogamy and fertility—a result which might be *à priori expected.*

(*d*) The coefficient of assortative mating ρ_{12} for parents weighted with their fertility differs sensibly from that of unweighted parents r_{12}. Generally the effect of a relation between homogamy and fertility is to increase the apparent coefficient of assortative mating.

(*e*) The coefficients of parental heredity are also modified when we take all and not a single representative of the offspring.

5. *Special Conclusions.*—These depend on how we define *homogamy.* When would the male and female be "alike"? Mr. Francis Galton, in the case of stature in man, reduces the female to the male equivalent by altering her stature in the ratio of mean male to mean female stature. In my paper on the Law of Ancestral Heredity* I give reasons for using as a factor of reduction the ratio of the male standard deviation to the female standard deviation. Mr. Galton's method and mine agree fairly closely in the case of man, for the coefficients of variation† of man and woman (*i.e.*, $100\sigma_1/m_1$ and $100\sigma_2/m_2$ in our present notation) are nearly equal for a considerable variety of organs. In either case we should understand by a homogamous union one in which the female organ reduced to the male equivalent was exactly equal to the male organ. Accordingly the ratio of p_1 to p_2 would be that of m_2 to m_1, or of σ_2 to σ_1 according to the hypothesis adopted. In the case of man, if either hypothesis be used, the other would be nearly satisfied. Hence, with a reasonable hypothesis as to what we mean by homogamy, it follows that—

(*a*) No progressive change in the mean would arise in a species owing to a relation between homogamy and fertility ($h_3 = 0$, since either $p_1/p_2 = m_2/m_1$ or σ_2/σ_1).

(*b*) With equipotency of hereditary influence in the parents, the race would not on my hypothesis alter its variability, and on Mr. Galton's hypothesis only by an extremely small quantity of the fourth order (if $r_{13} = r_{23}$, then by (vi) Σ_3 differs from σ_3 by a term of the order $r_{13}{}^2(p_1\sigma_1 - p_2\sigma_2)^2$).

(*c*) The coefficient of assortative mating will be increased. For if $p_1\sigma_1/s = p_2\sigma_2/s = \tau$, then

$$\rho_{12} = \frac{r_{12} + \tau^2(1 - r_{12}{}^2)}{1 + \tau^2(1 - r_{12}{}^2)},$$

which is greater than r_{12}. If $r_{12} = 0$, then $\rho_{12} = \tau^2/(1 + \tau^2)$, or a relation between homogamy and fertility would produce an apparent correlation between husband and wife, if we weighted them with their fertility, although they exercised no selective mating. This increase of ρ_{12} is in complete agreement with the result obtained for the coefficient

* 'Roy. Soc. Proc.,' vol. 62, p. 390.

† 'Phil. Trans.,' A, vol. 187, p. 276.

of assortative mating in the paper* "Data for the Problem of Evolution in Man. III."†

(*d*) The parental coefficient of heredity will generally be increased by taking all instead of a single one of the offspring.

For example, putting $r_{12} = 0$, $p_1\sigma_1/s = p_2\sigma_2/s = \tau$ as before, we find for equipotency—

$$\rho_{12} = \tau^2/(1+\tau^2), \qquad \rho_{31} = \rho_{32} = r_{31}\sqrt{(1+2\tau^2)/(1+\tau^2)}.$$

Thus
$$\rho_{31} = r_{31}\sqrt{1+\rho_{12}},$$

showing how the spurious coefficient of assortative mating modifies the coefficient of inheritance.

6. Thus I think it will be clear that *Reproductive Divergence* has not an effective existence. More generally *Reproductive Selection*, unless we suppose *ab initio* a fertility distribution with two modes (which is not given by homogamy, and wants, in any case, a special explanation), will not produce differentiation. It can produce, as I have often stated, progressive change. So far as I can yet see, differentiation must involve natural selection, and one can only appeal to reproductive selection as a means, but I think an effective means, of maintaining a differentiation already brought about by Darwin's fundamental factor in evolution.

Mr. Vernon, in his first paper, states that given a relationship between homogamy and fertility, then reproductive divergence "is capable of mathematical demonstration. This we will now proceed to afford" (p. 182).

In his second paper, he gives what he terms "the mathematical basis of the theory more fully" (p. 404). I venture to think that the whole of his treatment is fallacious. In the first paper he neglects the Law of Regression, and he thinks this justifiable, but it is not so. In

* 'Roy. Soc. Proc.,' vol. 66, p. 30.

† Assuming equipotent hereditary influence of father and mother for stature, we have from the above paper—

$$\rho = 0{\cdot}1783, \qquad r_{12} = 0{\cdot}0931, \text{ and}$$
$$\tau^2 = (\rho_{12}-r_{12})/\{(1-\rho_{12})(1-r_{12}^2)\} = 11{\cdot}9626.$$

Hence $p_1\sigma_1/s = p_2\sigma_2/s = \tau = 3{\cdot}4587$. Thus from the equation for y we find

$$\frac{y}{s} = 3{\cdot}4587\left(\frac{m_1+x_1}{\sigma_1} - \frac{m_2+x_2}{\sigma_2}\right).$$

If $X_1 = m_1+x_1$ = stature of father, $X_2 = m_2+x_2$ = stature of mother, we have for the relation between fertility and homogamy

$$n = n_0 e^{-\frac{11{\cdot}9626}{2}\left(\frac{X_1}{\sigma_1}-\frac{X_2}{\sigma_2}\right)^2}.$$

This will suffice to indicate how such relations can be numerically investigated.

the second paper he takes an arithmetical example based on 205 families. His results, if correct, would only show a flattening of the frequency-curve, an increased variability, and not a divergence or differentiation. But I have shown that the tendency is really to a decreased variability, and on examination it will be found that the differences on which Mr. Vernon bases his conclusions are all of the order of the probable errors of his results! Apart from this, however, the whole of his argument on pp. 405–6 seems to me invalid; we cannot proceed by a vague threefold classification such as he adopts; and he nowhere introduces, so far as I can see, the difference in height of the parents which must be the essential feature of the whole argument.

That Mr. Vernon has shown a relationship between homogamy and fertility in his 'Phil. Trans.' memoir is of high value, but I hold that such cannot help us in the *slightest* degree to dispense with the fundamental factor of Darwinian evolution, namely, natural selection.*

April 5, 1900.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of notice sent to the Fellows, an election was held to fill the vacancy upon the Council caused by the decease of Mr. G. J. Symons.

The statutes relating to the election of the Council, and the statute relating to the election of a Member of Council upon the occurrence of a vacancy, were read, and Professor Carey Foster and Sir E. Ommanney having been, with the consent of the Society, nominated scrutators, the votes of the Fellows present were taken and Mr. W. H. M. Christie, Astronomer Royal, was declared duly elected.

The following Papers were read:—

I. "On the Weight of Hydrogen desiccated by Liquid Air." By LORD RAYLEIGH, F.R.S.

* Since the above paper was sent to the Royal Society—

(*a*) The relationship of eye-colour to fertility in both man and woman has been investigated for several thousand cases; while there appears to be some correlation between eye-colour and fertility, homogamous unions do not appear to be the more fertile. The numbers will be eventually published;

(*b*) Mr. Vernon has sent me a letter stating that, on further investigation, he has modified his views on *Reproductive Divergence.*

II. "Combinatorial Analysis.—The Foundations of a New Theory." By Major MACMAHON, F.R.S.

III. "Über Reihen auf der Convergenzgrenze." By E. LASKER. Communicated by Major MACMAHON, F.R.S.

IV. "Extinct Mammalia from Madagascar. I.—*Megaladapis insignis*, sp.n." By C. I. FORSYTH MAJOR. Communicated by Dr. WOODWARD, F.R.S.

V. "The Kinetic Theory of Planetary Atmospheres. Part I." By G. H. BRYAN, F.R.S.

VI. "Observations on the Effect of Desiccation of Albumin upon its Coagulability." By J. B. FARMER. Communicated by Mr. H. T. BROWN, F.R.S.

VII. "Further Note on the Influence of the Temperature of Liquid Air on Bacteria." By Dr. A. MACFADYEN and S. ROWLAND. Communicated by LORD LISTER, P.R.S.

The Society adjourned over the Easter Recess to Thursday, May 10th.

"Mathematical Contributions to the Theory of Evolution. VII.—On the Application of certain Formulæ in the Theory of Correlation to the Inheritance of Characters not capable of Quantitative Measurement." By KARL PEARSON, F.R.S., with the assistance of Miss ALICE LEE, D.Sc., University College, London. Received August 5,—Read, November 16, 1899. Withdrawn, re-written, and again presented March 29, 1900.

(Abstract.)

(1) Many characters are such that it is very difficult if not impossible to form either a discrete or a continuous numerical scale of their intensity. Such, for example, are skin, coat, or eye-colour in animals, or colour in flowers. In other cases as in the amount of shading, degree of hairiness, &c., it might be possible by counting scales or hairs to obtain a numerical estimate of the character, but the labour in the case of several hundreds or a thousand individuals becomes appalling. Now these characters are some of those which are commonest, and of which it is generally possible for the eye at once to form an appreciation. A horse-breeder will classify a horse as brown, bay, or chestnut; a mother classify her child's eyes as blue, grey, or brown without hesitation and within certain broad limits correctly. It is clear that if the theory of correlation

can be extended so as to readily apply to such cases, we shall have much widened the field within which we can make numerical investigations into the intensity of heredity, as well as much lessened the labour of collecting data and forming records.

The extension of theory required for such investigations is provided in a separate memoir. It is found that the sole conditions for applying this theory are: (1) that an order of intensity must exist even if there be no quantitative scale; (2) that the correlation must be supposed normal. If these assumptions are made, individuals may even be classified into only two groups of less and greater intensity, and the correlation still found. For example, the correlation between stature and hair-colour could be found by classifying all individuals simply into short and tall, light and dark haired, although for convenience of judgment a medium class in each case might be introduced. For the purpose of ascertaining the relative variability of the characters involved, this third or medium class at least must be introduced and a ninefold division made of the correlation table. In the introduction to the present memoir the probable errors of all the quantities involved are considered, and illustrations given of their values for selected cases.

(2) The bulk of the memoir, however, is concerned with the application of this theory to two special cases, those of inheritance of the coat-colour of horses* and of the eye-colour of men.

There are three recognised chief types of inheritance: the blended heritage, the exclusive heritage, and the particulate heritage, to which latter two may possibly be added the reversionary heritage as a modifying factor.

In the blended heritage the character of the parents and the ancestry in the direct line are in the average offspring mingled in certain proportions. This heritage seems in broad lines to be described by the law of ancestral heredity.

In exclusive heritage the offspring takes the character of one parent to the exclusion of that of the other. While in blended heritage, reversion becomes very difficult, if not impossible, to distinguish from exceptional variation, here reversion becomes an easily detected feature; and studies on reversion ought if possible to deal with exclusive heritage. Lastly, in particulate heritage, we have a mixture, not a blend,

* The twelve tables of coat-colour inheritance have been extracted for me by Mr. Leslie Bramley-Moore out of Weatherby's Studbooks. He first pointed out to me the difficulties attending my method of proportioning, which led to my withdrawing and rewriting this paper. The twenty-four tables of eye-colour inheritance I have extracted from eye-colour data most generously placed at my disposal by Mr. Francis Galton. The arithmetic on these tables is chiefly due to Miss Alice Lee, D.Sc., but Mr. L. N. G. Filon, M.A., Mr. Bramley-Moore and Miss C. D. Fawcett, B.Sc., have given us friendly aid.

of the parental characters as in the case of a blue eye streaked with brown, eyes of two different colours, a piebald horse, &c. The occasional appearance of particulate, where we are accustomed to blended heritage, appears to be sometimes attributed to reversion.

Neither coat-colour in the horse nor eye-colour in man appear to obey the law of ancestral heredity.

(3) In coat-colour we find the horses lighter than the mares, but a secular change appears to be going on, and thoroughbred colts and fillies of to-day are more alike in colour than those of two generations back. The horse is somewhat more variable than the mare. The laws of inheritance are in excellent accord with what we should expect from the theory of exclusive inheritance without reversion; they are incompatible with the law of ancestral heredity. The only important divergence occurs in grandparental correlation.

(4) In eye-colour we find man lighter than woman, but a secular change is going on, and men and women to-day are more alike in eye-colour than they were two generations back. This change in eye-colour is possibly due to a correlation between eye-colour and fertility in woman. Man is somewhat more variable than woman. The laws of inheritance are not in accord with what we might expect from the law of ancestral heredity. They are definitely divergent from it, but agree well with exclusive heritage without reversion to ancestral types. Here again the grandparental correlation appears to be anomalously large. Great diversity exists, however, between the intensity of inheritance as exhibited by different lines of descent. For the first time in this memoir, I believe, the strength of heredity for the eight grandparental and the eight avuncular relationships is investigated. The results obtained enable us, at least for eye-colour in man, to make the following statements:—

(i) Let A and B be the grades of relationship, of which A refers to the older generation, and A and B may be of either sex. Then the variability of all the A's which have female B's is invariably greater than the variability of all the A's which have male B's. In other words, women while less variable than men, come of more variable stock.

(ii) The younger generation takes as a whole more strongly after its male than its female ascendants and higher collaterals.

(iii) The younger generation is more highly correlated with an ascendant or higher collateral reached by a line passing through one sex only than by a line which changes sex.

(iv) Men are slightly more highly correlated with their ascendants and higher collaterals than women are.

(5) The memoir concludes by insisting on the need for a wide determination of the intensity and form of inheritance for a great variety of characters in many types of life. Until this has been achieved, "*plasmic mechanics*" are merely hypothetical explanations of pheno-

mena,* of which we have as yet no sufficient cognizance. They strive to reach the generalisation of a Newton, without the numerical foundations laid by a Tycho Brahé and a Kepler.

"On the Retinal Currents of the Frog's Eye, excited by Light and excited Electrically." By AUGUSTUS D. WALLER, M.D., F.R.S. Received and Read March 29, 1900.

(Abstract.)

I. Introduction.

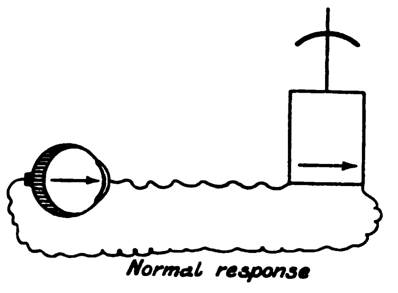
II. Plan of experiment.

III. Results.

1. A fresh normal eyeball manifests positive current, which gradually declines to zero, and becomes reversed.

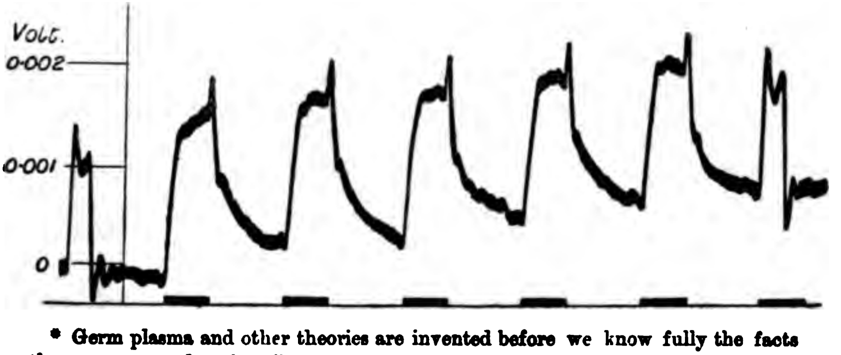
2. On exposure to light the normal current, whether positive or negative, undergoes a positive variation.

FIG. 1.



3. The magnitude of the response to light increases with the duration of illumination.

FIG. 2.



* Germ plasma and other theories are invented before we know fully the facts they are supposed to describe.

4. The magnitude of the response to light increases with th strength of illumination.

5. With lapse of time—or immediately in consequence of partia injury, the character of the response to light alters its type. Thre stages are to be recognised in accordance with the state of th retina, as (*a*) fresh; (*b*) transitional; (*c*) stale.

In the first stage the response is positive.

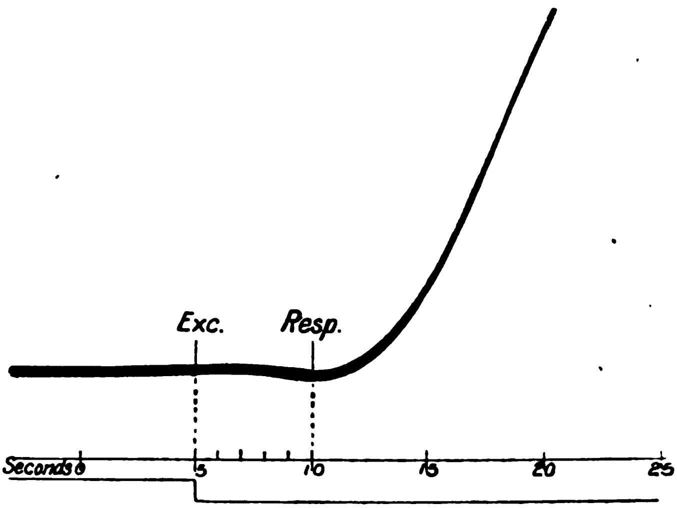
In the second stage the response is mixed.

In the third stage the response is negative.*

Theory.—Simultaneous opposite electrical effects are aroused in th retina by light.

6. The interval of time between stimulus and response is much i excess of a physiological latent period. In the second stage thi "period of hesitation" may amount to five seconds.

FIG. 3.



7. Under the influence of carbonic acid the response to light unde goes diminution or abolition followed by secondary augmentation.

8. In consequence of tetanisation in either direction the norma current becomes strongly positive (or less negative). This positiv change gradually subsides.

9. The positive response to light subsequent to such tetanisatio is much augmented, and the negative response is diminished.

* I have made a few experiments on mammalian eyes (cat), justifying th statement that the response immediately after removal is positive, and that at later period it is negative.

10. During tetanisation of moderate strength and of whatever direction the normal current becomes positive (or less negative). This positive change gradually subsides.

11. Strong single induction shocks of whatever direction arouse prolonged positive after-effects, that gradually subside.

12. Single condenser discharges (2 to 10 M.F., 1 to 7 volts), of whatever direction, arouse prolonged positive after-effects.

13. In consequence of gentle massage of the eyeball, the normal current becomes strongly positive (or less negative). This positive change gradually subsides.

14. In consequence of gentle massage of the eyeball, the positive response of the 1st stage gives place to a negative response (*vide supra*, 5).

15. Fatigue—*i.e.*, diminution of response by reason of previous activity—is less pronounced in the case of the retina than in that of muscle. It is manifested in nearly the same degree to stimulation by light, and to stimulation by tetanising currents.

16. The positive response to light (2), the positive effect of tetanisation (10), and the positive after-effect of condenser discharges (13) are suppressed by anæsthetics (ether and chloroform) and by rise of temperature (to 40—45°). The suppression may be permanent or temporary. An anæsthetised like a dead eyeball tested by currents, as in 11 and 12, manifests only polarisation currents negative in direction to the exciting currents. Tetanisation, as in 10, gives only polarisation effect in the direction of the break shocks negative to the direction of the make shocks.

IV. Conclusions.

"Observations on the Effect of Desiccation of Albumin upon its Coagulability." By J. BRETLAND FARMER, M.A., Royal College of Science, London. Communicated by Dr. H. T. BROWN, F.R.S. Received March 21,—Read April 5, 1900.

It has been known for some time that it is possible, under certain circumstances, to expose seeds to the influence of high temperatures without thereby necessarily destroying their power to germinate. Some experiments in this direction were conducted at the Royal Gardens, Kew, some years ago by Dr. Morris, but the results, although of much interest, do not appear to have been published. However, the seeds were exposed to the action of boiling water, and even to a higher temperature in an oven, without losing their ability to germinate when the ordeal was over.

It has been noticed, in heating seeds in water, that if the seed-coat

through any cause becomes ruptured, or if it softens and swells, the seeds which are thus affected are incapable of manifesting any further evidence of vitality. It appears to me that a fair inference to be drawn from these facts is that the admission of water to the living cells is a potent factor in bringing about their death.

Jodin* has recently communicated some facts which point to the same conclusion. He exposed seeds of pea and cress to a temperature of 98° C., and found that unless great care had been previously exercised to ensure the dryness of the seeds, they were all killed. When they had been previously dried he succeeded in subsequently germinating 30 per cent. of the peas and 60 per cent. of the cress seeds. Perhaps the disproportion in favour of the latter may, at least in part, be ascribed to their small size, and consequently to the less difficulty in sufficiently drying the seeds.

It would seem to follow from what has been said that the instability of the complex molecular structure of which living organisms are made up, may be lessened by appropriate desiccation, but the substances concerned are too complex to render themselves readily accessible to inquiry. It appeared, however, that it might be worth while to study the effects of desiccation on albumin from this point of view. Albumin is not only a highly complex proteid, and perhaps in some respects akin to protoplasm itself, but it is one which gives tolerably definite heat reactions. It is in connection with the last-mentioned point that the new facts in this paper are specially concerned.

It is of course known that albumin in a watery solution is readily coagulated on heating to a certain temperature. This temperature, however, is not necessarily constant for even one type of albumin, doubtless owing to the readiness with which it undergoes change. Thus albumin obtained from different hens' eggs will often be found to coagulate at different temperatures, and the differences appear, in part at any rate, to be connected with the age of the egg. I have found in the case of freshly-laid eggs, that the characteristic opalescence which marks the early stages of coagulation may set in as low as 60° C., the clotted coagulum being fully formed at 64° C. The heat was applied by means of a large water-bath, so as to ensure its being as uniform as possible. Another sample of albumin from a different egg tried simultaneously and under the same conditions, only exhibited opalescence at 65·6° C., and coagulated completely at 68° C.

The albumin on which most of my experiments were made, was obtained from Merck, of Darmstadt, and was sent as dried egg-albumin. It readily dissolved in water, with the exception of a little flaky insoluble portion, which was filtered off. The solution had a low coagulation-point, the opalescence appearing at 60° C., and the clot at 62° C.

* 'Comptes Rendus,' 1899.

to 63° C.* Filtering off the clot and testing the filtrate at higher temperatures yielded no further coagulation.

If a sample of this (dry) albumin be placed in a flask, the mouth of which is furnished with a cork and attached to a set of drying tubes, and the temperature of the flask raised to 80° C., a short exposure, of at any rate two to three hours, is enough to completely alter the albumin. The object of the drying tubes is to prevent any more moisture than is already present in its substance reaching the albumin from the steam or from any other source. Thus heated, the albumin is found to have become insoluble in water, and in fact to have undergone a change corresponding to coagulation.

If, however, the albumin be carefully *dried* before being subjected to these conditions, the results are quite different. For the present purpose it was found to be sufficient to expose a thin layer of albumin in a glass dish to a temperature of 52—55° C. in an incubator. This ensures a very thorough desiccation. The process may be hastened by introducing a vessel of sulphuric acid, though this precaution was not found to be necessary. Thus dried, the albumin loses its shellac or glue-like appearance, and easily crumbles to very small particles.

On comparing the solubility and coagulability of this specially dried material with the ordinary sample, no difference could be detected in any respect.

Numerous experiments were made with this dried material, of which the following may be taken as typical. It may be added that the results throughout were almost surprisingly uniform in the different experiments made.

A sample of the specially dried albumin was introduced in a flask so as to form a thin layer over the bottom. The flask was connected with drying tubes filled with calcium chloride, and with phosphorus pentoxide. The flask was warmed and cooled rapidly several times, in order to cause the contained air to circulate through the drying tubes.

The temperature of the flask was then raised in a brine bath to 102° C., and kept at this temperature during the whole of one day (six hours). Next day, and without opening or disturbing the apparatus, the temperature was again raised to 107° C., and finally to 110° C. It was maintained between these limits for seven hours; thus the contents of the flask had been for thirteen hours exposed to a temperature of considerably over 100° C.

On testing the albumin it was found to be soluble in water, and in no way, as far as could be observed, did it differ from the unheated material. On gradually warming the solution side by side with a

* It is of course known that several factors affect the coagulation point. The figures given represent those obtained in my experiments, which were all kept as uniform as possible so as to eliminate the factor of variability.

similar solution of the unheated albumin, both became opalescent at a temperature of 60° C., and both were completely coagulated at 62° C.

It thus appears that, if precautions are taken to ensure appropriate desiccation, it is possible to heat albumin for, at any rate, thirteen hours to a temperature varying between 102—110° C. without producing any obvious change in its ultimate molecular (or micellar ?) structure. It made no difference to the result whether the heat was gradually or rapidly applied. Thus, in one experiment, the temperature was raised from 50° C. to 103° C. in fifteen minutes, and in other examples the flask was withdrawn from the hot bath, cooled, and suddenly re-immersed. How much higher the temperature could be raised without producing an obvious effect, I am not prepared to say; nor did I investigate the action (if any) which might possibly be produced by a much longer exposure to heat within the limits already mentioned. This formed no part of my object, which was primarily to try to get a point of comparison between the complex seed and the simpler but still very complex proteid.

Other experiments were made in order to test the sensitiveness of the albumin to small quantities of moisture.

For this purpose, two flasks attached to drying tubes were used, one of them serving as a control experiment, and remaining unopened until the end. The other was opened three times, and a small sample taken out each time. By this means the ordinary air of the room obtained complete access to the albumin. The duration of the experiment was ten hours. The first sample was withdrawn after the flasks had been heated to 102° C. for three hours; it dissolved and coagulated normally. A second sample was withdrawn after three hours more, and it was found that whilst it dissolved and became opalescent on heating to 60° C., the coagulation change did not at once set in, but the opalescent solution became more milky and of a deeper fog-yellow by transmitted light, finally coagulating at about 68° C. A third sample taken out at the close of the experiment (*i.e.*, four hours after the last opening of the flask) also dissolved, became slightly opalescent at about 64° C., but did not coagulate even at 90° C., although the opalescent milkiness became very pronounced. Viewed by transmitted light, the solution was translucently yellow. Even boiling failed to produce anything which could be fairly termed a coagulum. It appeared probable that the admission of watery vapour had permitted the inception of the changes which normally, at high temperatures, result in coagulation; but in this case they were arrested, some precursor of alkali-albumin being probably produced, as is often the case on slowly coagulating albumin solutions. Under these circumstances, however, the entire mass of the albumin had undergone this change. This supposition turned out to be correct, for the addition of a trace of acetic acid at once caused the solution to

be susceptible to coagulation at about 60—62° C.* Hence it is fair to infer that although the slight amount of moisture introduced during the opening of the tube did not suffice to enable complete coagulation to occur, it did permit the early changes to begin, and to slowly, and in a modified way, to affect the entire mass. This experiment was repeated several times, and always with the same result.

It seems difficult, in the light of the foregoing observations, to resist the inference that in the complete absence of moisture albumin may be reduced to a state of relative molecular (or micellar) immobility; the rearrangements which, in the presence of water and at a sufficiently high temperature, normally take place in its ultimate structure being held in abeyance during the suspension of the essential condition of the presence of sufficient moisure. The substance is brought, so to speak, into a static condition; chemical or physico-chemical change is inhibited, just as is an interaction between phosphorus and oxygen when conditions of complete dryness obtain. It is tempting to extend these considerations to the case of seeds and spores, *e.g.*, of certain bacteria, and to ask whether similar conclusions may not be fairly assumed to obtain there, for it may well be a fact that the protoplasm, like the albumin, which is at any rate akin to it, when sufficiently desiccated withstands conditions which otherwise would certainly promote chemical disintegration. They, too, appear to be reduced to a "static" condition by drying, and the researches of Romanes† indicated no measurable chemical change as proceeding in them under these circumstances; and, again, the investigations of Brown and Escombe,‡ and of Sir W. Thiselton-Dyer,§ have also rendered it difficult to believe, when subjected to the other end of the scale of temperature, that any metabolism can really be proceeding. In these cases the molecular machinery of life is all present and intact, but the *manifestation of vitality*, as measured by chemical movement and by the change in the condition of energy, is absent. But such a state differs widely from death, seeing that when the conditions favourable to the continuous progress of those reactions which are associated with vitality are restored, the organism proceeds to work in the normal manner once more. Similarly the albumin heated in the desiccated form retains, instead of changing, that particular molecular condition which enables it, on restoring the essential conditions of moisture, to coagulate in a normal fashion when heated to a suitable degree of temperature.

* A solution of albumen treated with a very small quantity of a dilute solution of potash undergoes a similar change. The substance formed is not true alkali-albumen, since no precipitate is produced on neutralising, and a coagulum on heating this neutralised solution.

† 'Proc. Roy. Soc.,' vol. 57.

‡ *Ibid.*, vol. 62.

§ *Ibid.*, vol. 65.

"On the Weight of Hydrogen desiccated by Liquid Air." By LORD RAYLEIGH, F.R.S. Received February 22,—Read April 5, 1900.

In recent experiments by myself and by others upon the density of hydrogen, the gas has always been dried by means of phosphoric anhydride; and a doubt may remain whether on the one hand the removal of aqueous vapour is sufficiently complete, and on the other whether some new impurity may not be introduced. I thought that it would be interesting to weigh hydrogen dried in an entirely different manner, and this I have recently been able to effect with the aid of liquid air, acting as a cooling agent, supplied by the kindness of Professor Dewar from the Royal Institution. The operations of filling and weighing were carried out in the country as hitherto. I ought, perhaps, to explain that the object was not so much to make a new determination of the highest possible accuracy, as to test whether any serious error could be involved in the use of phosphoric anhydride, such as might explain the departure of the ratio of densities of oxygen and hydrogen from that of 16 : 1. I may say at once that the result was negative.

Each supply consisted of about 6 litres of the liquid, contained in two large vacuum-jacketed vessels of Professor Dewar's design, and it sufficed for two fillings with hydrogen at an interval of two days. The intermediate day was devoted to a weighing of the globe *empty*. There were four fillings in all, but one proved to be abortive owing to a discrepancy in the weights when the globe was empty, before and after the filling. The gas was exposed to the action of the liquid air during its passage in a slow stream of about half a litre per hour through a tube of thin glass.

I have said that the result was negative. In point of fact the actual weights found were $\frac{1}{10}$ to $\frac{2}{10}$ milligrams *heavier* than in the case of hydrogen dried by phosphoric anhydride. But I doubt whether the small excess is of any significance. It seems improbable that it could have been due to residual vapour, and it is perhaps not outside the error of experiment, considering that the apparatus was not in the best condition.

"The Kinetic Theory of Planetary Atmospheres." By G. H. BRYAN, Sc.D., F.R.S. Received March 15,—Read April 5, 1900.

(Abstract.)

The application of the kinetic theory to the atmospheres of planets dates from the paper of Waterston, who gave an investigation based on the then only possible assumption of equal velocities for all molecules, an assumption since known as Clausius' law. Of later papers reference is due in especial to Dr. Johnstone Stoney's memoir "Of Atmospheres on Planets and Satellites,"* in which the test of permanence of a gas in the atmosphere of a planet is made to depend on the ratio of its velocity of mean square to that relative velocity which would enable a suitably projected body to escape from the planet's attraction. If it be admitted, as Dr. Stoney assumes, that helium cannot exist in our atmosphere, it follows that vapour of water cannot exist on Mars.

The author's object has been to investigate the logical conclusions obtained by applying the Boltzmann-Maxwell distribution to the atmospheres of planets. In 1893 calculations were made, having special reference to the absence of atmosphere from the Moon, but these took no account of axial rotation. When this cause is taken into account, the distribution of co-ordinates and *relative* velocities of the molecules is found to be the same as if the planet were at rest, and "centrifugal force" applied to the system. The surfaces of equal density are of the forms originally investigated by Edward Roche, of Montpellier, and they cease to be closed surfaces when passing to the outside of the point on the equatorial plane where centrifugal force just balances the planet's attraction. Calling the surface through this point the "critical surface," the density of molecular distribution over this surface must be very small to ensure permanence. The ratio of the density at the planet's surface to the density at the critical surface has been called the "critical density ratio," and the author calculates its logarithm for particular gases at different temperatures on the various planets. The use of this logarithm has the advantage that the calculation can at once be extended to any gas at any temperature.

The high value obtained in the case of helium considered in reference to the earth, appears to afford abundant proof that if helium existed in our atmosphere it would possess a very high degree of permanence at ordinary temperatures. To test this point further, a calculation is made of the total rate at which molecules would flow across the critical surface, this rate being regarded as a superior limit to the

* 'Trans. R. Dublin Soc.'

rate at which the planet would lose its atmosphere, since it takes no account of molecules which describe free paths beyond the limit and fall back again. To further exhibit the results in a tangible form, the rate of flow is estimated by the number of years in which the total amount of gas escaping across the critical surface would be equal to the amount of the gas in a layer covering the surface of the planet to the depth of 1 cm. This measure is independent of the actual quantity of the gas under consideration existing in the atmosphere, since, if this quantity be increased, the rate of flow across the critical surface and the amount of gas present in the surface layer 1 cm. thick will be increased in the same proportion.

If a gas of molecular weight 2, such as helium, be supposed to exist in the earth's atmosphere, the loss in question would occupy $3{\cdot}5 \times 10^{36}$ years at $-73°$ C., 3×10^{19} years at 27°, $8{\cdot}4 \times 10^{10}$ years at 127° C., 6×10^{5} years at 227° C., and 222 years at 327° C.

If we halve the absolute temperatures we have the conditions applicable to hydrogen, the losses in question therefore taking place in $8{\cdot}4 \times 10^{10}$ years at $-73°$ C., 6×10^{5} years at $-23°$ C., and 222 years at 27° C.

For water vapour on Mars, the corresponding results are $1{\cdot}2 \times 10^{33}$ years at $-73°$, $1{\cdot}9 \times 10^{16}$ years at 27°, $2{\cdot}4 \times 10^{9}$ years at 127°, $4{\cdot}3 \times 10^{5}$ years at 227° and 106 years at 327°.

These figures indicate that helium cannot practically escape from our atmosphere at existing temperatures, nor can vapour of water escape from the atmosphere of Mars. A leakage may and undoubtedly does take place which may appear considerable when estimated by the number of actual molecules escaping, but it is wholly inappreciable relative to the mass of gas left behind.

At a future time I propose to examine the corresponding results, based on the hypothesis that the atmosphere of a planet is distributed according to the adiabatic instead of the isothermal law.

"Combinatorial Analysis.—The Foundations of a New Theory." By Major P. A. MacMahon, R.A., D.Sc., F.R.S. Received March 19,—Read April 5, 1900.

(Abstract.)

The object of the paper is to exhibit the processes of the infinitesimal calculus and of the calculus of finite differences as combinatorial processes. A large class of problems can be dealt with by designing on the one hand a function, and on the other hand an operation, in *such wise* that when the operation is performed upon the function a

number results which enumerates the combinations with which the problem is concerned. The problems to which the method is applicable are those which are, directly or indirectly, associated with lattices, and it is remarkable that, for the most part, they are such as have been hitherto regarded as unassailable by the processes of pure mathematics.

If a problem be presented for solution, it may be a difficult matter to design the function and the operation, the combination of which furnishes the solution. The plan here adopted is to consider certain functions and operations, and then to inquire into the nature of the associated problems. The author has attempted to place the method on a sure foundation, to give illustrative examples of gradually increasing complexity, and to indicate some promising lines of future investigation. The only published work connected with the subject is the author's paper, entitled "A New Method in Combinatory Analysis, with application to Latin Squares and Associated Questions," which will be found in the 'Trans. Camb. Phil. Soc.,' vol. xvi, Part IV, p. 262.

"Über Reihen auf der Convergenzgrenze." Von EMANUEL LASKER, Dr. Philos. Communicated by Major MACMAHON, F.R.S.
Received March 15,—Read April 5, 1900.

(Abstract.)

The essay is divided into three chapters.

A limit operation of any kind, for instance, simple or multiple integration; the formation of infinite sums or products; or any combination of these operations gives rise to certain typical considerations which form the subject of the first chapter. To fix the ideas, let

$$F(x) = u_1 + u_2 + \ldots\ldots + u_n + \ldots\ldots,$$

where the u_i are analytical functions of x; and let $x = \xi$ be a point on the curve which forms the boundary of the region of convergence of the series. Let C be a curve within the region of convergence terminating in $x = \xi$. In that case $F(\xi)$ is generally different from

$$\lim. F(x),$$
$$(\lim. x = \xi),$$

x varying on the curve C. It is a matter of the greatest importance to examine the relations between these two mathematical conceptions. If $u_1 + u_2 + \ldots\ldots + u_n + \ldots\ldots$ converges where $x = \xi$, $F(\xi)$ has a definite

meaning, but it does not thence follow that $\underset{(\lim. x = \xi)}{\lim. F(x)}$ has also a definite meaning.

But whenever the sum $U_1 + U_2 + \ldots\ldots + U_n + \ldots\ldots$ of the maxima values, which $u_1, u_2, \ldots\ldots u_n, \ldots\ldots$ assume upon C, is convergent, the expression

$$\underset{(\lim. x = \xi)}{F(x)}$$

has always a definite limit which coincides with $F(\xi)$. It is very easy to derive from this theorem a multitude of others. For instance, the maximum value of $x^n(1-x)^\lambda$ being

$$\frac{n^n \lambda^\lambda}{(n+\lambda)^{n+\lambda}},$$

λ being positive and x varying within the circle whose centre is 0, and whose radius is unity

$$(1-x)^\lambda \sum_{n=0,\ldots\infty} c_n x^n = \sum_{n=0,\ldots\infty} c_n x^n (1-x)^\lambda$$

has, for lim. $x = 1$, zero as limit whenever $\Sigma\, c_n/n^\lambda$ converges.

If $u_1 + u_2 + \ldots\ldots + u_n + \ldots\ldots$ be divergent when $x = \xi$, $\underset{(\lim. x = \xi)}{F(x)}$ will have ∞ as limit, if a certain condition be satisfied; viz., the curves described by u_n, x varying upon C, should lie in an angle ω, less than π, whose vertex is 0. This criterion is principally important on account of its connection with a certain theorem, the "type theorem."

Two sequences

$$u_1, u_2, \ldots\ldots u_n, \ldots\ldots$$

$$v_1, v_2, \ldots\ldots v_n, \ldots\ldots$$

are said to be of the same "type" if u_n/v_n, lim. $n = \infty$, lim. $x = \xi$, tends uniformly towards a finite limit ρ different from zero. If then $u_1 + u_2 + \ldots\ldots + u_n + \ldots\ldots$ be divergent when $x = \xi$ and the u_n satisfy the above condition

$$\frac{u_1 + u_2 + \ \ldots\ldots \ + u_n}{v_1 + v_2 + \ \ldots\ldots \ + v_n} \quad (\lim. x = \xi)$$

will tend towards ρ.

The second chapter deals with an application of the type theorem. A power series $\sum_{n=0,\ldots\infty} c_n x^n$ whose coefficients are such that

$$\frac{c_0 + c_1 + \ \ldots\ldots \ + c_n}{n^\lambda}$$

has for indefinitely increasing values of n, as limit ρ, tends multiplied by $(1 - x)^\lambda$, towards the limit $\rho\, \Gamma(\lambda + 1)$. This proposition, which follows immediately from the type theorem, gives for $\lambda = 0$ Abel's theorem; for $\lambda = 1$, the theorem of Frobenius ('Crelle,' 1878); and leads to consequences in regard to the integrals of linear differential equations with rational coefficients. The principal result of this chapter, is that the singularities of a function, defined by a power series $\Sigma c_n x^n$, can be found and their nature analysed by the examination only, for values of n tending towards ∞, of the sequence c_n and of other sequences derived from this one.

A new conception is introduced in the third chapter, that of an "asymptotical region." An asymptotical region encloses always a point $x = a$, of essential singularity, of a function $F(x)$ and consists of an ordinary region enclosing a with an infinity of co-holes in it, not enclosing a, but approaching it indefinitely. The object of the author is to throw some light on the manner in which a function behaves in the vicinity of a point of essential singularity. It is shown that if

$$F(x) = \Sigma \frac{b_n}{(x - c_n)} m_n,$$

a being ∞, and the m_n denoting finite integers, an asymptotical region can generally be constructed in which lim. $F(x) = 0$ if lim. $x = \infty$; and that this proposition is convertible. The new conception is applied to the theory of transcendental integral functions as founded by Weierstrass, Laguerre, Poincaré, &c. If $G(x)$ denotes a function of class zero, a certain asymptotical region will belong to G'/G in the above sense. If $H(x) = G(x) + c_1 G'(x) + c_2 G''(x) + \ldots\ldots$ where the c_i are any constants, such that the power series $\underset{(i = 1, \ldots\ldots \infty)}{\Sigma c_i t^i}$ has a non-vanishing radius of convergence, $H(x)$ will again be a transcendental integral function of class zero; and the asymptotical region belonging to $H(x)$ will be the same as that belonging to $G(x)$.

"Further Note on the Influence of the Temperature of Liquid Air on Bacteria." By ALLAN MACFADYEN, M.D., and S. ROWLAND, M.A. Communicated by LORD LISTER, P.R.S. Received April 3,—Read April 5, 1900.

In a previous communication* it was shown that no appreciable influence was exerted upon the vital properties of bacteria when exposed for 20 hours to the temperature of liquid air (−183° C. to

* 'Roy. Soc. Proc.,' February 1, 1900.

$-192°$ C.). Further experiments have since been made in which the organisms were again exposed to the temperature of liquid air for a much longer period, viz., seven days.

The organisms employed were *B. typhosus*, *B. coli communis*, *B. diphtheriæ*, *B. proteus vulgaris*, *B. acidi lactici*, *B. anthracis* (sporing culture), *Spirillum choleræ asiaticæ*, *Staphylococcus pyogenes aureus*, *B. phosphorescens*, a Sarcina, a Saccharomyces, and unsterilised milk.

Instead of being exposed as formerly on the actual media in which they were growing, the organisms were submitted to the cooling process in the form of a broth emulsion in hermetically sealed fine quill tubing. This allows of complete immersion, and effects a considerable economy in the amount of liquid air used, besides greatly facilitating manipulation. The liquid air was kindly furnished by Professor Dewar, and the experiment was conducted in his laboratory.

In the course of the experiment, the loss by evaporation of the liquid air was made up by adding fresh portions from time to time. In this way the temperature of about $-190°$ C. was maintained uninterruptedly through the whole period of the experiment. At the same time considerable care had to be taken in conducting the first cooling, in order to avoid fracture of the quill tubes. A preliminary cooling was therefore effected by means of solid CO_2. After the expiration of a week, the tubes were removed with cork-tipped forceps, and placed in a strong glass vessel till thawing was complete. The tubes were then opened, and the contents transferred to suitable culture media. In each case, a direct microscopical examination was made to detect any possible structural changes.

It is a remarkable fact that, notwithstanding the enormous mechanical strain to which the organisms must have been exposed, a strain far exceeding in amount any capable of being produced hitherto by direct mechanical means, not the slightest structural alteration could be detected.

The sub-cultures made at the conclusion of the experiment grew well, and in no instance could any impairment in the vitality of the organisms be detected. In one or two instances only, growth was slightly delayed, an effect which might have been due to other causes. The photogenic bacteria grew and emitted light, and the samples of milk became curdled.

The above experiments show that bacteria can be cooled down to $-190°$ C. for a period of seven days without any appreciable impairment of their vitality.

It has not yet been possible to undertake the experiments with liquid hydrogen.

Report of the Kew Observatory Committee for the Year ending December 31, 1899.

The operations of the Kew Observatory, in the Old Deer Park, Richmond, Surrey, are controlled by the Kew Observatory Committee, which is constituted as follows:—

Mr. F. Galton, *Chairman.*

Captain Sir W. de W. Abney, K.C.B., R.E.
Prof. W. G. Adams.
Captain E. W. Creak, R.N.
Prof. G. C. Foster.
Prof. J. Perry.
The Earl of Rosse, K.P.
Prof. A. W. Rücker.
Dr. R. H. Scott.
Mr. W. N. Shaw.
Lieut.-General Sir R. Strachey, G.C.S.I.
Rear Admiral Sir W. J. L. Wharton, K.C.B.

The work at the Observatory may be considered under the following heads:—

I. Magnetic observations.
II. Meteorological observations.
III. Seismological observations.
IV. Experiments and Researches in connexion with any of the departments.
V. Verification of instruments.
VI. Rating of Watches and Marine Chronometers.
VII. Miscellaneous.

I. Magnetic Observations.

The Magnetographs have been in constant operation throughout the year, and the usual determinations of the Scale Values were made in January.

The ordinates of the various photographic curves representing Declination, Horizontal Force, and Vertical Force were then found to be as follows:—

Declinometer: 1 cm. $= 0° 8'{\cdot}7$.

Bifilar, January, 1899, for 1 cm. $\delta H = 0{\cdot}00051$ C.G.S. unit.

Balance, January, 1899, for 1 cm. $\delta V = 0{\cdot}00052$ C.G.S. unit.

2 D 2

In the case of the Vertical Force instrument it was found necessary to re-adjust the magnet, and at the same time its sensibility was slightly altered, after which the scale value was again determined with the following result:—

Balance, January 26th, 1899, for 1 cm. $\delta V = 0{\cdot}00049$.

With regard to magnetic disturbances, no very large movements have been registered during the year. Some of the principal oscillations recorded took place on the following dates:—

January 28—29; February 12; March 10, 22, and 23;
April 18—19; May 3—5; June 27—29;
September 26—27; and October 23.

The hourly means and diurnal inequalities of the magnetic elements for 1899, for the quiet days selected by the Astronomer Royal, will be found in Appendix I. A correction has been applied for the diurnal variation of temperature, use being made of the records from a Richard thermograph as well as of the eye observations of a thermometer placed under the Vertical Force shade.

The mean values at the noons preceding and succeeding the selected quiet days are also given, but these of course are not employed in calculating the daily means or inequalities.

The following are the mean results for the entire year:—

Mean Westerly Declination............	16° 57′·1.
Mean Horizontal Force	0·18393 C.G.S. unit.
Mean Inclination	67° 14′·7.
Mean Vertical Force.....................	0·43852 C.G S. unit.

In September, in consequence of an accident, the two dip needles long in use had to be replaced by two others obtained from Mr. A. W. Dover in 1897. As a careful comparison of the data obtained before and after the accident showed that the difference of the inclinations given by the new and old needles if existent was less than the probable error of observation, no correction has been applied.

Observations of Absolute Declination, Horizontal Intensity, and Inclination have been made weekly, as a rule.

A table of recent values of the magnetic elements at the Observatories whose publications are received at Kew will be found in Appendix IA.

A course of practical instruction in the taking of magnetic observations has been given to Mr. Henkel, of Markree Observatory, and to Lieutenants Nares and Waugh, of the Royal Navy. The method of reducing the vertical force curves has been explained to Mr. Kitto, Superintendent of Falmouth Observatory.

Captain Creak, R.N., made some preliminary experiments with a modified form of Fox Circle, and Captain Denholm Fraser, R.E.,

experimented on the relative merits of silk and phosphor bronze as the suspension for magnets.

Open scale magnetographs, devised by Mr. W. Watson for the purpose of testing the disturbing action of electric railways, were in operation for a few days in December under Mr. Watson's supervision.

Dr. van Rijckevorsel visited the Observatory in June and September, and observed with his magnetic instruments, in pursuance of his scheme for the intercomparison of standard instruments at various Observatories.

Dr. L. A. Bauer also took magnetic observations in September and November with instruments belonging to the U.S. Coast and Geodetic Survey.

Advice has been given to Captain Fraser, R.E., with respect to the equipment for a magnetic survey of India, and to the Surveyor General, Wellington, New Zealand, in relation to the erection of a magnetic observatory in New Zealand, and the carrying out of a magnetic survey there. At the request of the Agent General, a complete set of magnetographs has been ordered from Mr. P. Adie for New Zealand, and the unifilar magnetometer and dip circle, previously lent to Melbourne Observatory, have been lent for two years to the New Zealand Government.

During the absence of Mr. T. W. Baker on inspection work during part of September and October, the magnetic work was intrusted to Mr. R. Forsyth, Royal College of Science, who was temporarily engaged for the purpose.

The magnetic work as a whole has been unusually onerous throughout the year, and it seems likely to continue heavy for some time, as an exceptionally large number of magnetic instruments have been ordered by foreign and colonial institutions with the expressed intention of having them verified at the Observatory.

Opportunities present themselves from time to time of getting valuable observations made by travellers and others if they are supplied with the necessary instruments. In order to be able to take advantage of such opportunities, by lending instruments to competent observers, when it may seem desirable to do so, the Committee have obtained a unifilar magnetometer and a dip circle from Mr. A. W. Dover. The expense was defrayed by a special grant, amounting to £86 5*s.*, from the Government Grant Committee.

II. Meteorological Observations.

The several self-recording instruments for the continuous registration of Atmospheric Pressure, Temperature of Air and Wet-bulb, Wind (direction and velocity), Bright Sunshine, and Rain have been maintained in regular operation throughout the year, and the

standard eye observations for the control of the automatic records have been duly registered. The monthly mean values are given in Appendix II.

The tabulations of the meteorological traces have been regularly made, and these, as well as copies of the eye observations, with notes of weather, cloud, and sunshine, have been transmitted, as usual, to the Meteorological Office.

With the sanction of the Meteorological Council, data have been supplied to the Council of the Royal Meteorological Society, the Institute of Mining Engineers, and the editor of 'Symons' Monthly Meteorological Magazine.'

Electrograph.—This instrument worked generally in a satisfactory manner during the year.

The "setting" of the electrometer needle, mentioned in last year's 'Report,' has been considerably reduced, and the working of the instrument improved, by the removal of the large glass cup, with a diameter of 100 mm.—used for holding the sulphuric acid—and the substitution for it of a small glass beaker, with a diameter of 40 mm., resting upon a disc of paraffin, and containing about 35 c.c. of acid. The acid and accumulated moisture is removed at frequent intervals.

Scale value determinations were made on January 24, May 12, July 21, and November 7, and in addition the potential of the battery has been tested weekly. Forty cells only have been employed throughout the year.

A battery of thirty-six Clark cells has been purchased from Messrs. Muirhead on behalf of the Meteorological Council, with the hope of thereby introducing greater certainty into the interpretation of the records.

With the sanction of the Meteorological Council, the electrograms for the year 1897 have been lent to Mr. C. T. R. Wilson, of Sidney-Sussex College, Cambridge.

Inspections.—In compliance with the request of the Meteorological Council, the following Observatories and Anemograph Stations have been visited and inspected:—Stonyhurst, Fleetwood, Armagh, Dublin, Valencia, Falmouth, and Fort William, by Mr. Baker; and Radcliffe Observatory (Oxford), Yarmouth, North Shields, Glasgow, Aberdeen, and Deerness (Orkney), by Mr. Constable.

III. Seismological Observations.

Professor Milne's "unfelt tremor" pattern of seismograph has been maintained in regular operation throughout the year; particulars of the time of occurrence and the amplitude in seconds of arc of the largest movements are given in Appendix III, Table I.

The disturbance (No. 145) on September 10 was particularly notice-

able; the range was beyond the limits of the instrument to record definitely, but the maximum exceeded 11 seconds of arc.

During November the action of the boom became sluggish, and the records for some time were doubtful. It was ultimately found, after consultation with Professor Milne, that a part of the edge of the agate cup resting on the pivot was scratched and jagged. This defect was remedied by moving the weight and tie piece round through 45°, and so bringing a different part of the agate cup on to the pivot. The general working has since been satisfactory.

The remarks made in last year's 'Report' as to the uncertainty of the time measurements still hold good, and no attempt is made to give these values to nearer than 0·1 minute.

A detailed list of the movements recorded from April, 1898, to March, 1899, was made and sent to Professor Milne, and will be found in the 'Report' of the British Association for 1899, "Seismological Investigations Committee's Report."

It is proposed to tabulate the disturbances for the remainder of 1899 in a similar manner.

IV. Experimental Work.

Fog and Mist.—The observations of a series of distant objects, referred to in previous 'Reports,' have been continued. A note is taken of the most distant of the selected objects which is visible at each observation hour.

Atmospheric Electricity.—The comparisons of the potential, at the point where the jet from the water-dropper breaks up, and at a fixed station on the Observatory lawn, referred to in last year's 'Report,' have been continued, and the observations have been taken three or four times every month.

Platinum Thermometry.—The results of the comparison of platinum and gas thermometers at Sèvres, referred to in last year's 'Report,' were worked up by Dr. Chappuis and Dr. Harker, and embodied in a paper which was read before the Royal Society in June and will appear in the 'Philosophical Transactions.'

The experiments which were begun in 1895 into the constancy and general behaviour of platinum thermometers have led to the accumulation of a large number of results. These have been dealt with by the Superintendent in a critical paper, which was recently read before the Royal Society.

Towards the end of the year an oil-bath was constructed, from the designs mainly of Dr. Harker, for the purpose of comparing thermometers at high temperatures. Some preliminary comparisons have already been made in it of a few German and English mercury standards with a platinum thermometer.

V. Verification of Instruments.

The subjoined is a list of the instruments examined in the year 1899, compared with a corresponding return for 1898 :—

	Number tested in the year ending December 31.	
	1898.	1899.
Air-meters	1	6
Anemometers	11	23
Aneroids	169	175
Artificial horizons	9	9
Barometers, Marine	122	92
,, Standard	58	85
,, Station	55	15
Binoculars	374	404
Compasses	44	43
Deflectors	3	6
Hydrometers	463	241
Inclinometers	5	9
Photographic Lenses	13	160
Magnets	2	3
Navy Telescopes	681	561
Rain Gauges	12	19
Rain-measuring Glasses	10	44
Scales	2	—
Sextants	750	876
Sunshine Recorders	15	6
Theodolites	26	24
Thermometers, Avitreous or Immisch's	10	5
,, Clinical	17,962	16,020
,, Deep sea	79	19
,, High Range	56	62
,, Hypsometric	38	39
,, Low Range	94	103
,, Meteorological	3,296	2,892
,, Solar radiation	2	—
,, Standard	66	104
Unifilars	6	5
Vertical Force Instruments	—	1
Declinometers	—	—
Total	24,434	22,051

Duplicate copies of corrections have been supplied in 97 cases.

The number of instruments rejected in 1898 and 1899 on account of excessive error, or for other reasons, was as follows :—

	1898.	1899.
Thermometers, clinical	173	149
" ordinary meteorological	92	78
Sextants	106	151
Telescopes	60	49
Binoculars	30	21
Various	26	14

Two Standard Thermometers have been constructed during the year.

There were at the end of the year in the Observatory, undergoing verification, 6 Barometers, 450 Thermometers, 24 Sextants, 150 Telescopes, 75 Binoculars, 6 Hydrometers, 2 Rain Measures, 2 Rain Gauges, and 2 Unifilar Magnetometers.

VI. Rating of Watches and Chronometers.

The number of watches sent for trial this year is slightly less than in 1898, the total entries being 469, as compared with 483 in the preceding year.

The "especially good" class A certificate was obtained by 78 watches. The highest number of marks obtained is a fraction lower than the highest obtained in 1898, but the average performance shows no falling off, as appears from the following figures showing the percentage number of watches obtaining the distinction "especially good," as compared to the total number obtaining class A certificates :—

Year	1894.	1895.	1896.	1897.	1898.	1899.
Percentage "especially good"	16·1	16·6	30·5	28·0	22·1	26·6

The 469 watches received were entered for trial as below :—

For class A, 362; class B, 86; and 21 for the subsidiary trial. Of these 19 passed the subsidiary test, 62 were awarded class B, and 293 class A certificates, while 95 failed from various causes to gain any certificate.

In Appendix IV will be found a table giving the results of trial of the 50 watches which gained the highest number of marks during the year. The highest place was taken by Messrs. S. Smith & Son, 9, Strand, London, with a keyless fusee tourbillon lever watch, No. 238–99, which obtained 88·7 marks out of a maximum of 100.

Marine Chronometers.—During the year, 56 chronometers have been entered for the Kew A trials; of these 34 gained certificates, and 22 failed.

No movements were sent in for the class B trials, and as the demand

for the B certificate has been very small indeed for some years pas the question of the retention of the class B trial seems to require co sideration.

The electrical contact-piece of the mean-time clock "French" faile in its action frequently in the early part of the year. This was four to be mainly due to the unequal wearing of the teeth of the old escap wheel. The clock was sent to Messrs. Dent, who fitted a new escap wheel, &c., and its general performance since has been much mo satisfactory.

VII. Miscellaneous.

Commissions.—The work under this heading has been of a ve varied character during the year. The following instruments ha been procured, examined, and forwarded to the various Observatori on whose behalf they were purchased:—

1 dip circle and 4 extra needles for St. Petersburg.
1 " " 1 pair " Toronto.
2 pairs dip needles for Upsala.
1 pair " " Mauritius.
1 Kew pattern self-recording Robinson anemometer and sheets, ai 1 pocket aneroid for St. Petersburg.
2 Kew standard thermometers and a barograph tabulator for Cola (Bombay).
A standard Fortin barometer, an astronomical globe, maximum a minimum thermometers, and an ozone cage for Mauritius.

Anemograph sheets, sunshine cards, and rain-gauge forms have be sent to Hong Kong and Mauritius; prepared photographic paper Batavia, Aberdeen, Fort William, and Valencia, for the Meteorologi Office; and to Hong Kong, Mauritius, Toronto, and Lisbon.

Gas Thermometer.—The instrument referred to in last year's 'Repo arrived at the Observatory in February. Prior to its receipt, Dr. J. Harker went over to Germany and was shown the methods of usi the gas thermometer adopted at the Reichsanstalt, Charlottenbu The Committee are much indebted to Dr. Kohlrausch and oth authorities of the Reichsanstalt for the courtesy shown by them this occasion. The cost of the instrument, including its carriage a Dr. Harker's expenses at Berlin, was borne by Sir A. Noble, who a kindly expressed his willingness to pay for the auxiliary applian required in gas thermometry. Owing to the want of a suital building in which to erect the gas thermometer, the Committee we unable to take full advantage of Sir Andrew's generous offer for t immediate present, and they have been obliged to leave it to th successors, the Executive Committee of the National Physical Labo

tory, to carry out the final arrangements for the installation of the gas thermometer.

Collimator Magnets.—A critical and experimental paper dealing with the data obtained in the verification of collimator magnets at the Observatory during the last forty years was prepared by the Superintendent, and has been published in the Royal Society's 'Proceedings.'

Discussion on Platinum Thermometry.—A discussion on platinum thermometry having been arranged for the British Association meeting at Dover, Dr. Harker attended, with the Committee's approval, and in concert with Dr. Chappuis gave a summary of their joint work at Sèvres.

Professor Carey Foster and Mr. Shaw also took part in the debate as well as the Superintendent, who had been instructed by the Committee to attend.

Compass-testing Regulations.—In consequence of representations by Mr. J. White, of Glasgow, the regulations for the testing of ships' compasses have been revised. In this process the Committee had the advantage of the advice of Lord Kelvin and Captain Creak, whose views were laid before a sub-committee appointed for the purpose.

At the request of the Danish Legation, the methods employed at the Observatory for the verification of compasses, sextants, and naval telescopes were shown to Commander Clausen, of the Royal Danish Navy, who has charge of the verification of naval instruments at Copenhagen.

National Physical Laboratory.—Parliament having, on the motion of Her Majesty's Ministers, voted a sum of money for the establishment of a National Physical Laboratory, to be under the management of a committee nominated by the Council of the Royal Society, the Royal Society have drawn up, and the Government have approved, a scheme for the organisation of the Laboratory. In accordance with this scheme, the Kew Observatory is incorporated with the National Physical Laboratory, and becomes part of the organisation thereof as from the 1st January, 1900. The Kew Observatory Committee as hitherto constituted ceases to exist at the same date, and its property is to be transferred to the Royal Society. The work of the Observatory will, however, proceed as heretofore, and will be carried on by the existing staff.

The scheme of organisation already mentioned constitutes an Executive Committee as the authority having the immediate management of the National Physical Laboratory, and this Committee includes at present six members of the Kew Observatory Committee. The scheme also provides for the appointment of a Director, who, subject to the authority of the Executive Committee, is to have sole control and direction of the officials of the National Physical Laboratory and of the work done within it. Mr. R. T. Glazebrook, F.R.S., has been appointed to this office.

The Kew Observatory Committee having been incorporated und the Companies Act, 1867, certain legal forms have to be complied wi in order to wind it up, transfer its property to the Royal Society, a put an end to its liabilities. The steps required for these purposes a being taken.

Inspection of the Observatory.—An inspection by the General Board the National Physical Laboratory was arranged for October 16th a 18th, when the Chairman and some other members of the Kew Co mittee attended at the Observatory to assist in showing it to th visitors. On the second occasion the Observatory was visited by fu twenty members of the General Board, including the Vice-Chairm of the Executive Committee and the Director of the National Physi Laboratory. By the courtesy of the Mid-Surrey Golf Club, arran ments were made for examining the most likely sites for buildi afforded by the Old Deer Park.

Library.—During the year the library has received publicatic from—

21 Scientific Societies and Institutions of Great Britain a Ireland,

103 Foreign and Colonial Scientific Establishments, as well as fr several private individuals.

The card catalogue has been proceeded with.

Audit, &c.—The accounts for 1899 have been audited by Messrs. ' B. Keen & Co., Chartered Accountants.

The balance sheet, with a comparison of the expenditure for t two years 1898 and 1899, is appended.

PERSONAL ESTABLISHMENT.

The staff employed is as follows:—

C. Chree, Sc.D., F.R.S., Superintendent.
T. W. Baker, Chief Assistant.
E. G. Constable, Observations and Rating.
W. Hugo, Verification Department.
J. Foster " "
T. Gunter " "
W. J. Boxall " "
G. E. Bailey, Accounts and Library.
E. Boxall, Observations and Rating.
G. Badderly, Verification Department, and six other Assistants.
A Caretaker and a Housekeeper are also employed.

In addition to the above, Dr. J. A. Harker has been employed in t capacity of special assistant to the Superintendent.

(Signed) G. CAREY FOSTER,
Interim Chairman.

List of Instruments, Apparatus, &c., the Property of the Kew Observatory Committee, at the present date out of the custody of the Superintendent, on Loan.

To whom lent.	Articles.	Date of loan.
G. J. Symons, F.R.S.	Portable Transit Instrument................	1869
The Science and Art Department, South Kensington.	Articles specified in the list in the Annual Report for 1893.......................	1876
Professor W. Grylls Adams, F.R.S.	Unifilar Magnetometer, by Jones, No. 101, complete............................	1883
	Pair 9-inch Dip Needles with Bar Magnets...	1887
Lord Rayleigh, F.R.S.	Standard Barometer (Adie, No. 655)	1885
Radcliffe Observatory, Oxford.	Black Bulb Thermometer *in vacuo*	1897
The Borchgrevink-Newnes Antarctic Expedition.	Dip Circle, by Barrow, No. 24, with four Needles and Bar Magnets...............	1898
The New Zealand Government.	Unifilar Magnetometer, by Jones, marked N.A.B.C., complete.....................	1899
	Dip Circle, by Barrow, with one pair of Needles and Bar Magnets...............	1899
	Tripod Stand	1899
C. T. R. Wilson, Esq., Cambridge.	Electrograms for 1897	1899

Kew Observatory. Account of Receipts and Payments (including those of the Liquidator since December 1st) for the year ending December 30, 1899.

RECEIPTS.

	£	s.	d.	£	s.	d.
To Balance from Year 1898				650	9	4
Royal Society:—						
Gassiot Trust. Annual payment	440	16	10			
,, ,, Income Tax returned	14	19	9			
				455	16	7
Meteorological Council:—						
Allowance	400	0	0			
Postages, &c.	5	10	6			
Set of Clark's Standard Cells	23	8	0			
				428	18	6
Tests:—						
Verification	1616	16	5			
Rating	516	11	0			
Lenses	42	8	10			
				2175	16	3
Researches:—						
London and South Western Railway Company—Compensation for broken bell-jar					10	0
Commissions executed for Colonial and Foreign Institutions, &c.				470	5	2
Rents				7	3	0
Dividends on India Stock				43	19	8
Gas Thermometer—Sir Andrew Noble's contribution				75	0	0
Government Grant Committee for Magnetic Instruments for Loan				86	5	0
				£4294	3	6

PAYMENTS.

	£	s.	d.	£	s.	d.
By Normal Observatory:—						
Salaries—Observations, Tabulations, &c.	349	15	3			
Incidental Expenses, Apparatus, Photographic Paper, &c.	74	18	11			
Proportion of Administration Expenditure	194	10	0			
				619	4	2
Researches:—						
Salaries	204	11	10			
Incidental Expenses, &c.	28	10	7			
Proportion of Administration Expenditure	389	0	0			
				622	2	5
Tests:—						
Salaries	969	18	0			
Incidental Expenses, &c.	156	12	0			
Proportion of Administration Expenditure	584	11	4			
				1711	1	4
Commissions:—						
Purchase of Instruments and Photographic Paper for Colonial and Foreign Institutions, &c.	340	10	6			
Proportion of Administration Expenditure	130	0	0			
				470	10	6
Gas Thermometer:—						
Apparatus and Carriage from Berlin	64	16	0			
Expenses of Dr. Harker's visit to Berlin	9	15	0			
				74	11	0
Mr. A. W. Dover—Magnetic Instruments for Loan				86	5	0
Balance—London and County Bank	766	14	9			
Awaiting Banking	19	5	6			
In hand (Petty Cash)	24	8	10			
				810	9	1
				£4294	3	6

Audited on behalf of the Royal Society and found correct,
18th January, 1900.
(Signed) W. B. KEEN, *Chartered Accountant.*

ADMINISTRATION EXPENDITURE.

Particulars.	£	s.	d.
Superintendent	500	0	0
First Assistant, Librarian, &c.	457	7	9
Rent, Fuel, &c.	96	4	4
Caretaker, Repairs, &c.	244	9	3
	£1298	1	4

Apportionment.	£	s.	d.
Observatory	194	10	0
Researches	389	0	0
Tests	584	11	4
Commissions	130	0	0
	£1298	1	4

ESTIMATED ASSETS.

	£	s.	d.	£	s.	d.
By Balance as per Statement				810	9	1
£1300 India 3½ per cent. Stock, value on January 1, 1900				1430	0	0
Payments due:—						
Meteorological Council—Allowance, Postages, &c.	100	15	4			
Test Fees	563	17	5			
Commissions, &c.	22	2	0			
				686	14	9
Stock:—						
Blank Forms and Certificates	53	16	5			
Standard Thermometers	79	8	0			
				133	4	5
				£3060	8	3

January 19th, 1900.

ESTIMATED LIABILITIES.

	£	s.	d.
To Administration accounts—Gas, Water, Repairs, &c.	34	15	10
Observatory accounts—Photographic Paper, &c.	6	18	1
Tests accounts	11	17	2
Commissions	17	16	2
Researches		3	3
Unspent balance of Grant for Seismograph	4	5	0
" " Sir Andrew Noble's contribution to Gas Thermometer		9	0
General Balance	2984	3	9
	£3060	8	3

(Signed) CHARLES CHREE,
Superintendent.

Comparison of Expenditure during the Years 1898 and 1899.

Expenditure.	1898.	1899.	Increase.	Decrease.
Administration :—	£ s. d.	£ s. d.	£ s. d.	£ s. d
Superintendent........	500 0 0	500 0 0		
First Assistant.........	333 8 0	331 18 0		1 10 0
Office.................	121 10 0	125 9 9	3 19 9	
Rent, Fuel, Lighting, &c.	87 16 6	96 4 4	8 7 10	
Caretaker............	68 18 0	68 18 0		
Incidental Expenses....	137 12 1	175 11 3	37 19 2	
	1249 4 7	1298 1 4	50 6 9	1 10 0
Normal Observatory:—				
Salaries—Observations, &c..................	336 15 6	349 15 3	12 19 9	
Incidental Expenses....	41 1 7	74 18 11	33 17 4	
Prop. Adm. Expenditure	187 10 0	194 10 0	7 0 0	
Researches :—				
Salaries................	158 8 0	204 11 10	46 3 10	
Incidental Expenses.....	64 9 2	28 10 7		35 18 7
Prop. Adm. Expenditure	375 0 0	389 0 0	14 0 0	
Tests :—				
Salaries..............	918 6 0	969 18 0	51 12 0	
Incidental Expenses....	222 9 5	156 12 0		65 17 5
Prop. Adm. Expenditure	499 4 7	584 11 4	85 6 9	
Commissions :—				
Purchases for Colonial Institutions, &c.	529 3 1	340 10 6		188 12 7
Prop. Adm. Expenditure	187 10 0	130 0 0		57 10 0
Seismograph...........	55 15 0			55 15 0
Gas Thermometer.......		74 11 0	74 11 0	
Magnetic Instruments for Loan		86 5 0	86 5 0	
Gross Expenditure.... (showing an increase of £8 2*s*. 1*d*.).	3575 12 4	3583 14 5	411 15 8	403 13 7
Extraordinary Expenditure.				
Normal Observatory :—				
Incidental Expenses....		23 8 0	23 8 0	
Researches :—				
Salaries..............	158 8 0	204 11 10	46 3 10	
Purchase of Apparatus, &c.	61 15 10	23 19 3		37 16 7
Commissions :—				
Purchases for Colonial Institutions, &c.......	529 3 1	340 10 6		188 12 7
Seismograph...........	55 15 0			55 15 0
Gas Thermometer.......		74 11 0	74 11 0	
Magnetic Instruments for Loan................		86 5 0	86 5 0	
	805 1 11	753 5 7	230 7 10	282 4 2
Leaving for Ordinary Nett Expenditure........... (showing an increase of £59 18*s*. 5*d*.).	2770 10 5	2830 8 10	181 7 10	121 9 5

APPENDIX I.

MAGNETICAL OBSERVATIONS, 1899.

Made at the Kew Observatory, Old Deer Park, Richmond, Lat. 51° 28′ 6″ N. and Long. $0^h\ 1^m\ 15^s{\cdot}1$ W.

The results given in the following tables are deduced from the magnetograph curves which have been standardised by observations of deflection and vibration. These were made with the Collimator Magnet K.C. I. and the Declinometer Magnet marked K.O. 90 in the 9-inch Unifilar Magnetometer by Jones.

The Inclination was observed with the Inclinometer by Barrow, No. 33, and needles 3½ inches in length.

The Declination and Force values given in Tables I to VIII are prepared in accordance with the suggestions made in the fifth report of the Committee of the British Association on comparing and reducing Magnetic Observations.

The following is a list of the days during the year 1899 which were selected by the Astronomer Royal, as suitable for the determination of the magnetic diurnal inequalities, and which have been employed in the preparation of the magnetic tables :—

January	1, 7, 10, 13, 27.
February	4, 5, 7, 8, 18.
March	4, 5, 26, 27, 30.
April	13, 15, 16, 21, 22.
May	13, 14, 24, 25, 29.
June	6, 7, 17, 25, 26.
July	15, 17, 22, 28, 29.
August	12, 16, 18, 19, 23.
September	5, 6, 7, 14, 20.
October	2, 3, 10, 20, 29.
November	2, 10, 16, 20, 27.
December	6, 11, 14, 15, 24.

Table I.—Hourly Means of the Declination, as determined fr

Hours	Preceding noon.	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
		(16° +) West					Winter.					
1899. Months.	′	′	′	′	′	′	′	′	′	′	′	′
Jan. ..	61·3	57·9	58·3	58·2	58·3	58·2	57·9	58·0	57·9	58·1	58·4	59·0
Feb. ..	61·4	58·0	58·1	58·1	58·0	57·9	58·0	58·0	58·1	58·5	59·1	60·1
March.	62·1	56·9	56·8	56·6	56·7	56·8	56·9	56·7	56·7	56·0	55·6	56·8
Oct. ..	59·7	55·3	55·3	55·3	55·2	55·1	55·0	54·8	54·2	53·5	53·8	55·2
Nov. ..	58·3	54·5	55·1	55·2	55·4	55·3	55·0	54·9	54·5	54·1	54·4	55·7
Dec. ..	57·5	55·0	55·1	55·1	55·2	55·3	55·3	55·1	54·9	54·9	55·0	55·5
Means	60·1	56·3	56·5	56·4	56·5	56·4	56·4	56·3	56·1	55·9	56·1	57·1
						Summer.						
	′	′	′	′	′	′	′	′	′	′	′	′
April..	61·5	56·9	57·0	57·2	57·1	56·7	56·6	55·9	54·8	53·6	54·2	56·3
May ..	62·3	57·0	57·0	56·8	56·6	56·1	54·9	53·9	53·0	53·1	54·7	57·2
June ..	61·6	56·8	56·7	56·5	56·5	55·7	54·3	53·0	52·6	52·6	53·3	55·5
July ..	60·2	56·4	55·8	55·7	55·4	55·0	53·9	53·6	53·6	53·1	54·1	55·7
Aug. ..	61·5	55·5	55·4	55·5	55·3	54·8	54·0	53·2	52·5	52·8	54·1	57·0
Sept...	61·6	56·1	56·0	56·2	55·9	55·7	55·3	54·7	53·6	53·4	54·4	56·4
Means	61·5	56·4	56·3	56·3	56·1	55·7	54·8	54·0	53·3	53·1	54·1	56·3

Table II.—Diurnal Inequality

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
					Summer Means.						
	′	′	′	′	′	′	′	′	′	′	′
	−0·7	−0·8	−0·8	−1·0	−1·5	−2·3	−3·1	−3·8	−4·0	−3·0	−0·8
					Winter Means.						
	′	′	′	′	′	′	′	′	′	′	′
	−0·8	−0·6	−0·6	−0·6	−0·6	−0·7	−0·8	−1·0	−1·2	−1·0	0·0
					Annual Means.						
	′	′	′	′	′	′	′	′	′	′	′
	−0·7	−0·7	−0·7	−0·8	−1·1	−1·5	−2·0	−2·4	−2·6	−2·0	−0·4

NOTE.—When the sign is + the
" " −

selected quiet Days in 1899. (The Mean for the Year = 16° 57′·1 West.)

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.	Succeeding noon
						Winter.							
′	′	′	′	′	′	′	′	′	′	′	′	′	′
60·5	60·3	59·3	58·9	59·3	59·0	58·8	58·7	58·4	58·2	58·0	58·1	58·0	61·1
61·1	61·0	60·7	59·7	58·8	59·3	59·1	58·9	58·5	58·0	58·0	58·0	57·8	61·1
62·2	63·2	62·8	61·0	59·2	58·4	57·9	57·5	56·9	57·2	57·0	56·9	57·1	62·2
58·6	59·5	59·3	58·2	57·1	56·5	56·2	56·1	55·8	55·6	55·5	55·1	55·2	58·9
57·8	57·8	57·1	55·9	55·6	55·6	55·5	55·3	54·9	54·6	54·7	54·7	54·7	57·9
57·0	57·3	56·6	56·1	55·7	55·5	55·3	55·0	54·9	54·7	54·8	54·9	54·9	57·8
59·5	59·9	59·3	58·3	57·6	57·4	57·1	56·9	56·6	56·4	56·3	56·3	56·3	59·8
						Summer.							
′	′	′	′	′	′	′	′	′	′	′	′	′	′
62·1	63·9	63·8	62·5	60·5	59·1	57·9	56·9	57·3	57·3	57·2	57·2	57·1	61·7
62·2	62·7	62·1	60·5	58·9	58·1	57·7	57·7	57·7	57·5	57·4	57·3	57·0	63·2
61·0	62·2	62·7	61·9	60·6	59·0	58·5	57·8	57·5	57·4	57·2	57·3	56·9	61·7
60·5	61·4	61·4	60·2	59·0	57·9	57·4	57·0	57·1	56·9	56·9	56·6	56·5	61·0
61·0	61·9	60·8	59·3	58·0	56·4	55·9	56·1	56·2	56·2	55·9	55·6	55·6	62·5
61·9	62·9	62·3	60·1	58·3	57·0	56·6	56·4	56·5	56·3	56·3	56·2	56·1	62·7
61·4	62·5	62·2	60·7	59·2	57·9	57·3	57·0	57·1	56·9	56·8	56·7	56·5	62·1

Declination as deduced from Table I.

Noon	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	M
						Summer Means.						
′	′	′	′	′	′	′	′	′	′	′	′	
+4·3	+5·4	+5·0	+3·6	+2·1	+0·8	+0·2	−0·2	−0·1	−0·2	−0·3	−0·5	−(
						Winter Means.						
′	′	′	′	′	′	′	′	′	′	′	′	
+2·5	+2·8	+2·2	+1·2	+0·6	+0·3	+0·1	−0·2	−0·5	−0·7	−0·7	−0·7	−(
						Annual Means.						
′	′	′	′	′	′	′	′	′	′	′	′	
+3·4	+4·1	+3·6	+2·4	+1·3	+0·5	+0·1	−0·2	−0·3	−0·5	−0·5	−0·6	−(

points to the west of its mean position.
„ east „ „

Table III.—Hourly Means of the Horizontal Force in C.G.S. units (corr
(The Mean fo

Hours	Preceding noon.	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
0·18000 +						Winter.						
1899. Months.												
Jan. ...	381	381	382	382	384	386	388	338	388	387	380	381
Feb. ...	380	385	384	383	384	385	386	388	389	388	383	381
March..	374	383	383	382	381	383	387	386	387	381	373	368
Oct. ...	387	405	406	405	405	405	407	406	405	398	392	387
Nov. ...	400	402	403	404	407	409	410	410	407	402	398	394
Dec. ...	404	407	406	407	409	410	411	411	411	410	408	406
Means..	388	394	394	394	395	396	398	398	398	394	389	386
						Summer.						
April...	363	386	386	386	387	386	388	386	383	378	372	366
May ...	370	394	393	393	390	390	387	384	376	367	362	361
June ...	376	399	397	396	395	394	395	391	387	381	377	376
July ...	381	397	396	396	397	397	395	391	388	383	381	379
Aug. ...	388	401	400	400	399	398	395	392	386	380	375	376
Sept. ...	381	404	405	404	403	402	401	397	390	383	374	371
Means..	377	397	396	396	395	394	393	390	385	379	374	372

Table IV.—Diurnal Inequality

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	
						Summer Means.						
	+ ·00005	+ ·00004	+ ·00004	+ ·00003	+ ·00002	+ ·00001	– ·00002	– ·00007	– ·00013	– ·00018	– ·00020	–
						Winter Means.						
	– ·00001	– ·00001	– ·00001	·00000	+ ·00002	+ ·00004	+ ·00004	+ ·00003	·00000	– ·00005	– ·00008	–
						Annual Means.						
	+ ·00002	+ ·00002	+ ·00002	+ ·00002	+ ·00002	+ ·00003	+ ·00001	– ·00002	– ·00007	– ·00012	– ·00014	–

NOTE.—When the sign i

for Temperature) as determined from the selected quiet Days in 1899. Year = 0·18393.)

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.	Succee noo
						Winter.							
384	388	388	384	380	384	385	386	385	384	383	383	384	38
382	387	389	387	385	385	386	387	389	389	388	388	389	38
372	378	382	383	388	382	383	385	384	383	384	385	384	37
389	393	399	402	402	405	406	407	408	408	408	407	408	39
397	403	406	407	409	410	411	410	408	408	407	407	407	39
406	408	406	408	409	411	412	412	411	411	410	410	411	41
388	393	395	395	395	396	397	398	397	397	397	397	397	39
						Summer.							
365	371	378	386	387	388	394	391	391	392	391	390	391	36
376	385	390	391	391	392	395	399	400	397	396	396	395	37
381	386	391	396	399	400	406	409	408	406	404	402	400	38
385	388	394	399	399	401	403	407	408	406	405	403	402	38
384	390	396	398	399	402	404	410	411	410	407	407	406	38
386	394	401	404	405	406	406	408	406	405	406	406	405	38
380	386	392	396	397	398	401	404	404	403	402	401	400	37

Horizontal Force as deduced from Table III.

Noon	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	
						Summer Means.						
− ·00012	− ·00006	·00000	+ ·00004	+ ·00005	+ ·00006	+ ·00009	+ ·00012	+ ·00012	+ ·00011	+ ·00010	+ ·00009	+
						Winter Means.						
− ·00006	− ·00002	+ ·00001	+ ·00001	·00000	+ ·00002	+ ·00003	+ ·00003	+ ·00003	+ ·00003	+ ·00002	+ ·00002	+
						Annual Means.						
− ·00009	− ·00004	·00000	+ ·00002	+ ·00003	+ ·00004	+ ·00006	+ ·00008	+ ·00007	+ ·00007	+ ·00006	+ ·00005	+

reading is above the mean.

Table V.—Hourly Means of the Vertical Force in C.G.S. units (corr
(The Mean for t

Hours.	Preceding noon.	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
	0·43000 +					Winter.						
1899. Months.												
Jan. ...	840	846	845	844	844	844	843	843	842	841	841	841
Feb. ...	841	844	843	843	842	843	842	842	841	840	841	841
March ..	848	866	865	864	864	863	862	861	862	861	859	855
Oct. ...	863	871	870	869	869	868	867	868	869	869	867	864
Nov. ...	820	825	826	827	826	826	826	824	825	825	823	822
Dec. ...	827	830	830	830	830	829	830	830	830	829	828	827
Means	840	847	847	846	846	846	845	845	845	844	843	842
						Summer.						
April ...	843	865	863	863	863	863	863	864	864	864	859	854
May ...	843	861	861	860	860	860	860	862	860	858	851	843
June ...	840	851	850	849	849	847	847	846	846	844	840	837
July ...	852	866	864	863	862	862	863	861	861	859	857	854
Aug. ...	841	856	855	855	854	854	854	854	854	851	847	841
Sept. ...	855	868	868	867	866	866	866	866	867	865	859	857
Means	846	861	860	860	859	859	859	859	859	857	852	848

Table VI.—Diurnal Inequality o

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	
						Summer Means.						
	+ ·00003	+ ·00002	+ ·00002	+ ·00001	+ ·00001	+ ·00001	+ ·00001	+ ·00001	– ·00001	– ·00006	– ·00010	–
						Winter Means.						
	+ ·00001	+ ·00001	·00000	·00000	·00000	– ·00001	– ·00001	– ·00001	– ·00002	– ·00003	– ·00004	–
						Annual Means.						
	+ ·00002	+ ·90001	+ ·00001	·00000	·00000	·00000	·00000	·00000	– ·00001	– ·00004	– ·00007	–

NOTE.—When the sign

for Temperature), as determined from the selected quiet Days in 1899.
Year = 0·43852.)

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.	Succeedi noon.
						Winter.							
841	842	845	847	847	848	846	846	846	845	845	844	844	839
841	839	840	841	842	842	842	843	841	842	841	842	842	841
851	853	861	864	871	874	872	872	871	869	868	866	864	848
861	862	864	871	873	872	871	871	871	871	870	870	870	857
825	828	831	832	831	831	830	829	828	828	827	827	827	821
828	830	831	833	834	834	834	834	838	833	833	833	832	825
841	842	845	848	850	850	849	849	848	848	847	847	847	839
						Summer.							
843	846	853	859	863	867	869	868	867	866	866	865	864	842
841	846	851	859	862	864	865	864	864	862	860	859	858	835
836	841	849	854	858	859	862	862	860	857	852	849	849	829
847	852	857	860	866	869	871	873	872	870	868	867	865	850
838	843	851	858	862	863	863	861	859	857	855	854	854	839
854	857	863	865	869	870	869	870	870	869	869	867	866	849
843	847	854	859	863	865	867	866	865	864	862	860	859	841

Vertical Force as deduced from Table V.

Noon	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mi
						Summer Means.						
− ·00015	− ·00010	− ·00004	+ ·00001	+ ·00005	+ ·00007	+ ·00009	+ ·00008	+ ·00007	+ ·00006	+ ·00004	+ ·00002	+ ·0
						Winter Means.						
− ·00005	− ·00003	·00000	+ ·00002	+ ·00004	+ ·00004	+ ·00003	+ ·00003	+ ·00002	+ ·00002	+ ·00002	+ ·00001	+ ·0
						Annual Means.						
− ·00010	− ·00007	− ·00002	+ ·00002	+ ·00005	+ ·00006	+ ·00006	+ ·00006	+ ·00005	+ ·00004	+ ·00003	+ ·00002	+ ·0

reading is above the mean.

Table VII.—Hourly Means of the Inclination, calculated from the Horizonta

Hours	Preceding noon.	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
		67° +					Winter.						
1899. Months.	′	′	′	′	′	′	′	′	′	′	′	′	′
Jan.....	15·2	15·4	15·3	15·2	15·1	15·0	14·8	14·8	14·8	14·8	15·2	15·2	15·2
Feb.....	15·8	15·0	15·1	15·1	15·0	15·0	14·9	14·8	14·7	14·7	15·1	15·2	15·3
March..	15·9	15·7	15·7	15·8	15·8	15·7	15·4	15·4	15·4	15·7	16·2	16·4	16·4
Oct.....	15·4	14·4	14·3	14·4	14·4	14·4	14·2	14·3	14·4	14·9	15·2	15·4	15·4
Nov. ...	13·4	13·4	13·3	13·3	13·1	12·9	12·9	12·8	13·0	13·4	13·6	13·8	13·9
Dec.....	13·3	13·2	13·3	13·2	13·0	12·9	12·9	12·9	12·9	12·9	13·0	13·1	13·2
Means..	14·7	14·5	14·5	14·5	14·4	14·3	14·2	14·2	14·2	14·4	14·7	14·9	14·9
							Summer.						
	′	′	′	′	′	′	′	′	′	′	′	′	′
April...	16·5	15·5	15·5	15·5	15·4	15·5	15·4	15·5	15·7	16·0	16·4	16·6	16·6
May....	16·0	14·9	15·0	14·9	15·1	15·1	15·3	15·6	16·1	16·6	16·7	16·6	16·4
June ...	15·5	14·3	14·4	14·4	14·5	14·5	14·4	14·7	15·0	15·3	15·4	15·4	15·2
July....	15·5	14·8	14·8	14·8	14·7	14·7	14·9	15·1	15·3	15·6	15·7	15·7	15·4
Aug. ...	14·7	14·3	14·3	14·3	14·4	14·4	14·6	14·8	15·2	15·6	15·8	15·6	15·3
Sept. ...	15·6	14·4	14·4	14·4	14·4	14·5	14·6	14·8	15·3	15·7	16·2	16·3	15·9
Means..	15·6	14·7	14·7	14·7	14·8	14·8	14·9	15·1	15·4	15·8	16·0	16·0	15·8

Table VIII.—Diurnal Inequality of the

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
						Summer Means.						
	′ −0·3	′ −0·2	′ −0·2	′ −0·2	′ −0·2	′ −0·1	′ +0·1	′ +0·5	′ +0·9	′ +1·1	′ +1·1	′ +0·9
						Winter Means.						
	′ +0·1	′ +0·1	′ +0·1	′ 0·0	′ −0·1	′ −0·3	′ −0·3	′ −0·2	′ 0·0	′ +0·3	′ +0·4	′ +0·5
						Annual Means.						
	′ −0·1	′ −0·1	′ −0·1	′ −0·1	′ −0·1	′ −0·2	′ −0·1	′ +0·1	′ +0·4	′ +0·7	′ +0·7	′ +0·7

NOTE.—When the sign is +

and Vertical Forces (Tables III and V). (The Mean for the Year = 67° 14′·7.)

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.	Succeedin noon.
Winter.													
′	′	′	′	′	′	′	′	′	′	′	′	′	′
15·0	14·8	14·9	15·2	15·4	15·2	15·1	15·0	15·1	15·1	15·2	15·2	15·1	14·7
15·1	14·8	14·6	14·8	15·0	15·0	14·9	14·9	14·7	14·7	14·7	14·8	14·7	15·1
16·1	15·7	15·7	15·7	15·9	16·1	15·9	15·8	15·8	15·9	15·8	15·6	15·6	16·0
15·2	15·0	14·6	14·6	14·7	14·5	14·4	14·3	14·2	14·2	14·2	14·3	14·2	14·7
13·7	13·4	13·3	13·2	13·1	13·0	12·9	12·9	13·0	13·0	13·1	13·1	13·1	13·6
13·2	13·1	13·3	13·2	13·1	13·0	13·0	12·9	13·0	13·0	13·1	13·1	13·0	12·8
14·7	14·5	14·4	14·5	14·5	14·5	14·4	14·3	14·3	14·3	14·4	14·3	14·3	14·5
Summer.													
′	′	′	′	′	′	′	′	′	′	′	′	′	′
16·3	16·0	15·7	15·4	15·4	15·5	15·1	15·3	15·3	15·2	15·2	15·3	15·2	16·4
15·6	15·1	14·9	15·0	15·1	15·1	15·0	14·7	14·6	14·7	14·7	14·7	14·7	15·2
15·1	14·9	14·8	14·8	14·5	14·4	14·1	13·9	13·9	14·0	14·0	14·0	14·2	14·9
15·1	15·0	14·8	14·5	14·7	14·6	14·6	14·4	14·3	14·3	14·4	14·5	14·5	15·1
14·9	14·7	14·5	14·5	14·6	14·4	14·3	13·8	13·7	13·7	13·9	13·8	13·9	14·9
15·2	14·8	14·5	14·4	14·4	14·4	14·3	14·2	14·3	14·4	14·3	14·3	14·3	15·4
15·4	15·1	14·9	14·8	14·8	14·7	14·6	14·4	14·3	14·4	14·4	14·4	14·5	15·3

Inclination as deduced from Table VII.

Noon	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mi
Summer Means.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+0·4	+0·1	−0·1	−0·2	−0·2	−0·2	−0·4	−0·6	−0·6	−0·6	−0·5	−0·5	−0·
Winter Means.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+0·3	0·0	0·0	0·0	+0·1	0·0	−0·1	−0·1	−0·1	−0·1	−0·1	−0·1	−0·
Annual Means.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+0·4	+0·1	−0·1	−0·1	0·0	−0·1	−0·2	−0·3	−0·4	−0·3	−0·3	−0·3	−0·

the reading is above the mean.

2 E 3

APPENDIX IA.

MEAN VALUES, for the years specified, of the Magnetic Elements at Observatories whose Publications are received at Kew Observatory.

Place.	Latitude.	Longitude.	Year.	Declination.	Inclination.	Horizontal Force. C.G.S. Units.	Vertical Force C.G.S. Units.
	° ′	° ′		° ′	° ′		
Pawlowsk	59 41 N.	30 29 E.	1897	0 25·6 E.	70 40·2 N.	·16514	·47078
Katharinenburg	56 49 N.	60 38 E.	1897	9 51·2 E.	70 40·0 N.	·17812	·50771
Kasan	55 47 N.	49 8 E.	1892	7 30·8 E.	68 36·2 N.	·18551	·47345
Copenhagen ...	55 41 N.	12 34 E.	1898	10 19·8 W.	68 43·0 N.	·17467	·44839
Stonyhurst	53 51 N.	2 28 W.	1898	18 21·9 W.	68 53·6 N.	·17260	·44713
Hamburg......	53 34 N.	10 3 E.	1896	11 36·7 W.	67 38·8 N.	·18061	·43921
Wilhelmshaven	53 32 N.	8 9 E.	1898	12 37·5 W.	67 47·4 N.	·18045	·44196
			1899	12 31·9 W.	67 45·0 N.	·18072	·44173
Potsdam	52 23 N.	13 4 E.	1898	10 5·0 W.	66 35·3 N.	·18794	·43408
Irkutsk........	52 16 N.	104 16 E.	1897	2 3·6 E.	70 12·4 N.	·20145	·55975
Utrecht	52 5 N.	5 11 E.	1897	14 5·2 W.	67 1·9 N.	·18511	·43676
Kew..........	51 28 N.	0 19 W.	1899	16 57·1 W.	67 14·7 N.	·18393	·43852
Greenwich*....	51 28 N.	0 0	1898	16 39·2 W.	67 11·9 N.	·18387	·43737
Uccle (Brussels)	50 48 N.	4 21 E.	1898	14 22·4 W.	66 17·0 N.	·18930	·43088
Falmouth	50 9 N.	5 5 W.	1898	18 37·5 W.	66 51·8 N.	·18627	·43571
Prague	50 5 N.	14 25 E.	1898	9 15 8 W.	—	·19906	—
St. Helier (Jersey).........	49 12 N.	2 5 W.	1899	17 3·7 W.	65 49·4 N.	—	—
Parc St. Maur (Paris)	48 49 N.	2 29 E.	1897	14 58·6 W.	64 59·6 N.	·19717	·42270
Vienna........	48 15 N.	16 21 E.	1898	8 24·1 W.	—	·20797	—
O'Gyalla (Pesth)	47 53 N.	18 12 E.	1897	7 44·3 W.	—	·21114	—
			1898	7 38·2 W.	—	·21114	—
			1899	7 33·9 W.	—	·21129	—
Odessa........	46 26 N.	30 46 E.	1897	4 47·3 W.	62 30·9 N.	·22039	·42372
Pola..........	44 52 N.	13 51 E.	1898	9 30·9 W.	—	·22111	—
Nice†	43 43 N.	7 16 E.	1898	12 8·2 W.	60 13·6 N.	·22349	·39065
			1899	12 4·0 W.	60 11·7 N.	·22390	·39087
Toronto.......	43 40 N.	79 30 W.	1897	4 53·0 W.	—	·16650	—
Perpignan	42 42 N.	2 53 E.	1896	13 55·3 W.	60 5·9 N.	·22398	·38948
Rome.........	41 54 N.	12 27 E.	1891	10 45·1 W.	58 4·6 N.	·2324	·3730
Tiflis	41 43 N.	44 48 E.	1896	1 53·7 E.	55 48·1 N.	·25670	·37775
Capodimonte (Naples)	40 52 N.	14 15 E.	1897	9 26·3 W.	56 31·4 N.	·24075	·36406
Madrid	40 25 N.	3 40 W.	1896	16 1·7 W.	—	—	—
			1897	15 56·9 W.	—	—	—
Coimbra.......	40 12 N.	8 25 W.	1897	17 32·3 W.	59 36·3 N.	·22658	·38628
			1898	17 27·9 W.	59 33·6 N.	·22691	·38613

* Observations taken on site of new magnetic pavilion. In case of Inclination 3-inch needles alone employed.

† In last year's table the Declination at Nice should be 12° 12·8′ (not 12° 18·8′).

APPENDIX IA—*continued.*

Place.	Latitude.	Longitude.	Year.	Declination.	Inclination.	Horizontal Force. C.G.S. Units.	Vertical Force. C.G.S. Units.
Washington ..	38° 55′ N.	77° 4′ W.	1894	3° 39′·9 W.	70° 34′·3 N.	·19979	·56646
Lisbon	38 43 N.	9 9 W.	1899	17 22·6 W.	57 58·4 N.	·23451	·37484
Zi-ka-wei	31 12 N.	121 26 E.	1896	2 18·1 W.	45 52·7 N.	·32676	·33693
Havana	23 8 N.	82 25 W.	1898	3 10·8 E.	52 30·7 N.	·31166	·40634
Hong Kong* ...	22 18 N.	114 10 E.	1898	0 22·6 E.	31 33·3 N.	·36607	·22481
Tacubaya......	19 24 N.	99 12 E.	1895	7 45·6 E.	44 22·2 N.	·33428	·32764
Colaba (Bombay)	18 54 N.	72 49 E.	1896	0 33·8 E.	20 55·6 N.	·37463	·14326
Manila........	14 35 N.	120 58 E.	1897	0 51·4 E.	16 33·2 N.	·37910	·11268
Batavia	6 11 S.	106 49 E.	1897	1 18·6 E.	29 37·8 S.	·36767	·20913
Mauritius	20 6 S.	57 33 E.	1897	9 43·6 W.	54 27·4 S.	·23900	·33452
Melbourne.....	37 50 S.	144 58 E.	1898	8 20·1 E.	67 22·4 S.	·23364	·56050

APPENDIX II.—Table I.

Mean Monthly Results of Temperature and Pressure. Kew Observatory.
1899.

Months.	Thermometer.								Barometer.*					Mean vapour-tension.
	Mean.	Means of—			Absolute Extremes.				Mean.	Absolute Extremes.				
		Max.	Min.	Max. and Min.	Max.	Date.	Min.	Date.		Max.	Date.	Min.	Date.	
1899.	°	°	°	°	°	d. h.	°	d. h.	ins.	ins.	d. h.	ins.	d. h.	in.
Jan.....	42·8	47·3	37·9	42·6	55·4	21 1 P.M.	28·8	6 2 A.M.	29·842	30·692	25 9 P.M.	28·841	2 8 A.M.	·225
Feb.	41·6	47·6	36·2	41·9	62·3	10 1 P.M.	21·9	28 1 "	29·909	30·729	28 9 A.M.	29·171	13 7 P.M.	·216
March..	39·9	48·1	32·3	40·2	59·8	31 3 "	22·5	21 7 "	30·097	30·687	1 10 "	29·019	9 4 A.M.	·195
April...	47·4	54·2	41·2	47·7	63·8	1 4 "	32·2	18 6 "	29·834	30·338	23 1 "	28·885	14 1 "	·251
May ...	51·5	59·9	43·3	51·6	71·3	31 4 & 5 P.M.	34·8	4 5 "	30·026	30·488	28 11 "	29·413	15 4 " & 10 "	·276
June...	60·9	70·7	51·0	60·9	80·6	5 4 P.M.	42·6	14 4 " 15 4 "	30·067	30·447	8 10 "	29·381	20 6 P.M.	·354
July ...	66·1	75·4	57·0	66·2	86·4	21 3 "	50·4	5 4 "	30·078	30·474	31 10 P.M.	29·523	1 11 A.M.	419
Aug....	65·7	75·8	56·4	66·1	87·3	15 3 "	48·2	22 5 "	30·096	30·463	1 0·5 A.M.	29·740	31 MIDT.	424
Sept....	57·8	66·1	49·8	58·0	84·2	5 5 "	35·2	29 5 "	29·864	30·249	11 7 "	29·262	30 4 A.M.	·344
Oct.....	48·6	56·6	41·5	49·1	62·2	1 2 "	31·0	14 7 "	30·074	30·445	21 9 P.M.	29·141	1 4 P.M.	·295
Nov. ...	47·3	52·5	41·4	47·0	60·8	4 2 "	27·8	30 7 "	30·198	30·729	17 10 A.M.	29·390	8 7 A.M.	·280
Dec. ...	37·0	41·1	32·4	36·8	54·0	6 9 "	21·3	14 6 "	29·910	30·578	3 10 "	28·436	29 5 P.M.	·193
Yearly Means	50·6	57·9	43·4	50·7	..	....	..	....	30·000	..	....	..	..	·289

* Reduced to 32° at M.S.L.

This table has been compiled at the Meteorological Office from values intended for publication in the volume of "Hourly Means" for 1899.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=overcast).	Rainfall.* Total.	Rainfall.* Maximum.	Rainfall.* Date.	Weather. Number of days on which were registered: Rain. ‡	Snow.	Hail.	Thunderstorms.	Clear sky.	Overcast sky.	Gales.§	Wind.† Number of days on which it was: N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.‖
1899.		ins.	ins.																	
January	6·4	2·385	0·420	13	19	1	1	..	7	14	5	1	7	..	..	6	10	5	2	2
February	5·4	2·020	0·380	6	9	1	..	..	10	10	5	1	2	8	4	6	4	1	2	7
March	5·4	0·560	0·140	25	10	3	..	..	7	8	1	4	1	6	1	1	7	6	5	6
April	7·5	2·375	0·320	24	20	2	..	..	1	16	..	3	..	1	1	3	9	8	5	4
May	6·2	1·465	0·445	14	11	..	..	1	3	9	1	5	6	4	1	5	8	2	..	4
June	5·2	1·610	0·520	28	6	..	..	1	8	9	..	7	4	4	3	5	1	3	3	4
July	5·6	0·610	0·140	10	9	..	..	3	6	9	..	5	1	3	..	4	5	8	5	5
August	4·5	0·445	0·190	29	6	..	..	..	7	4	..	1	3	12	5	..	5	2	3	1
September ...	6·0	2·165	0·900	29	14	..	..	2	3	8	..	3	1	1	..	3	7	12	3	4
October	4·9	1·975	1·055	27	8	..	..	..	9	9	..	3	4	10	..	4	6	2	2	12
November ...	8·0	3·980	{1·350 {1·250	3 5	8	..	..	..	1	19	1	2	2	1	1	6	9	7	2	6
December ...	7·5	1·255	0·260	1	15	1	..	..	4	20	1	4	3	7	4	3	5	3	2	8
Totals and means.	6·0	20·845			135	8	1	7	66	135	14	39	34	57	20	46	76	59	34	63

* Measured at 10 A.M. daily by gauge 1·75 feet above ground. † As registered by the anemograph.
‡ The number of rainy days are those on which 0·01 inch rain or melted snow was recorded.
§ In a "gale" the mean wind velocity has exceeded 35 miles an hour in at least one hour of the twenty-four.
‖ In a "calm" the mean wind velocity for the twenty-four hours has not exceeded 5 miles an hour.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Bright Sunshine.				Maximum temperature in sun's rays. (Black bulb *in vacuo*.)			Minimum temperature on the ground.			Horizontal movement of the air.*		
	Total number of hours recorded.	Mean percentage of possible sunshine.	Greatest daily record.	Date.	Mean.	Highest.	Date. †	Mean.	Lowest.	Date. ‡	Average hourly velocity.	Greatest hourly velocity.	Date.
1899.	h. m.		h. m.		deg.	deg.		deg.	deg.		miles.	miles.	
January	70 48	27	6 18	9	69	90	22	31	18	25	14·9	48	21
February	80 54	29	7 48	22	79	105	10	28	13	26	12·4	46	13
March	119 12	33	9 24	22	90	116	29	23	9	25	9·1	37	29
April	131 0	31	12 24	19	106	118	25, 29	35	20	19	11·2	33	6
May	219 12	46	14 54	31	119	128	18	36	20	4	10·8	35	17
June	253 6	51	15 6	15	124	135	26, 28	43	28	15	8·3	24	20, 30
July	264 42	53	14 30	30	131	147	9	51	41	14	8·5	26	1
August	261 42	58	13 12	21	125	137	15	48	38	21, 22	10·2	32	4
September	167 0	44	12 18	4	115	138	5	44	28	29	9·4	26	22
October	103 48	31	9 42	13	89	119	3	35	22	14	7·5	34	15
November	51 36	19	8 18	1	73	96	1, 2, 8	34	21	18	9·2	44	3
December	40 30	17	6 36	31	56	84	1	25	6	14	9·0	40	29
Totals and Means	1763 30	37	..	..	98	..	..	36	..	..	10·0	..	..

* As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground, the original factor 3 being used.

† Read at 10 A.M., and entered to previous day. ‡ Read at 10 A.M., and entered to same day.

APPENDIX III.—Table I.

Register of principal Seismograph Disturbances. 1899.

No. in Kew register.	Date.	Commencement of P.T.'s.*	Duration of P.T.'s.*	1st maximum.	2nd maximum.	Amplitude in seconds of arc.	Total duration of disturbance.
		h. m.	m.	h. m.	h. m.		h. m.
49	Jan. 14	2 58·2	27·2	3 26·5	3 28·2	1·03	1 11·5
50	„ 22	8 22·2	5·6	8 29·1	—	0·77	0 27·5
52	„ 24–25	23 47·7	43·4	0 35·6	0 42·6	2·44	2 59·6
89	April 12	17 47·2	29·6	18 35·1	—	0·46	1 44·6
112	June 5	15 15·4	30·0	15 46·2	15 55·2	0·58	1 11·6
114	„ 14	11 28·6	22·5	11 52·0	11 59·1	1·90	2 9·4
124	July 12	1 55·3	10·8	2 12·3	2 13·3	1·12	1 4·0
125	„ 14	13 31·4	21·9	13 54·0	13 56·8	1·60	3 35·6
142	Sept. 4	0 33·6	8·3	1 3·0	1 7·3	7·49	2 49·2
144	„ 10	17 15·3	7·3	17 50·1	17 53·5	2·18	1 39·0
145	„ 10	21 1·6	58·9	22 20–21	22 25·6	exceeded 10·80	3 0·0
149	„ 20	2 16·7	4·6	2 21·8	2 27·5	3·20	1 22·8
150	„ 23	11 23·3	20·2	11 46·7	11 49·8	0·70	1 19·2
151	„ 23	14 1·8	19·0	14 24·3	14 26·3	0·70	1 12·4
152	„ 29	17 23·2	5·3	17 35·5	17 45·7	0·51	2 18·8
168	Nov. 23	10 1·0	9·8	10 10·8	10 11·8	1·04	1 4·5
169	„ 24	19 5·5	19·1	19 41·4	19 43·4	0·69	0 59·5
179	Dec. 31	10 59·2	8·9	11 13·7	—	0·88	0 53·8
180	„ 31	20 37·1	18·7	20 58·2	21 2·7	0·50	0 57·7

* P.T.'s = preliminary tremors. The times recorded are G.M.T.; midnight = 0 or 24 hour
The figures given above are obtained from the photographic records of a Milne Horizont
Pendulum; they represent E—W displacements.

APPENDIX IV.—Table I.

RESULTS OF WATCH TRIALS. **Performance of the 50 Watches which obtained the highest number of marks during the year.**

Watch deposited by	Number of watch.	Escapement, balance spring, &c.	Mean daily rate. Pendant up.	Pendant right.	Pendant left.	Dial up.	Dial down.	Mean variation of daily rate. ±	Mean change of rate for 1° F.	Difference between extreme gaining and losing rates.	Marks awarded for Daily variation of rate.	Change of rate with change of position.	Temperature compensation.	Total Marks.
											0—40	0—40	0—20	0—100.
			secs.	secs.	secs.	secs.	secs.	secs.	secs.	secs.				
S. Smith & Son, London	238-99	D.r., fusee, s.o., "Tourbillon" lever	+0·6	−0·0	−0·4	−1·1	−0·2	0·4	0·03	5·0	32·6	38·2	17·9	88·7
Montandon-Robert, Geneva	1078	D.r., g.b., s.o.	−1·5	−1·9	−2·9	−2·0	−1·6	0·3	0·05	4·5	33·2	38·5	16·5	88·2
S. Yeomans, Coventry	76656	S.r., g.b., s.o., "Karrusel"	+0·9	+2·1	+1·3	+0·9	+1·6	0·4	0·04	4·0	31·5	38·4	18·0	87·9
Montandon-Robert, Geneva	1112	D.r., g.b., s.o., "Karrusel"	−1·9	−1·6	−1·5	−1·6	−2·4	0·3	0·06	3·7	33·0	38·9	15·8	87·7
S. Yeomans, Coventry	76153	S.r., g.b., s.o., "Karrusel"	+3·2	+3·0	+3·7	+2·0	+3·9	0·5	0·01	4·2	29·7	37·9	19·3	86·9
Chas. Frodsham & Co., London	08647	D.r., fusee, s.o., "Tourbillon" lever	+1·8	+1·1	+1·0	+0·9	+3·0	0·4	0·04	5·2	31·9	37·3	17·5	86·7
Fridlander, Coventry	14846	S.r., g.b., s.o.	+0·4	−0·2	−0·3	+2·2	+2·6	0·3	0·05	3·7	34·8	35·4	16·4	86·6
" "	25534	S.r., g.b., s.o., "Karrusel"	−0·0	−0·3	−0·3	−3·8	+2·7	0·3	0·04	8·2	34·5	34·5	17·5	86·5
S. Yeomans, Coventry	76657	S.r., g.b., s.o., "Karrusel"	−1·8	−1·3	−1·2	−1·9	−3·4	0·4	0·05	4·2	32·3	37·6	16·6	86·5
Montandon-Robert, Geneva	1095	D.r., g.b., s.o., minute repeater	−1·5	−3·1	−1·0	−1·9	−0·8	0·4	0·06	5·0	32·6	37·3	16·1	86·0
Ehrhardt, Ltd., Birmingham	272874	S.r., g.b., s.o.	−1·0	−2·0	−0·5	+0·1	+0·8	0·5	0·01	4·5	29·3	36·8	19·9	86·0
Montandon-Robert, Geneva	1060	D.r., g.b., s.o.	+3·1	+0·7	+1·9	+3·5	+2·8	0·4	0·05	4·2	32·4	36·5	16·9	85·8
S. Smith & Son, London	1899-1	G.b., s.o., "Tourbillon" chronometer	−1·5	+0·4	−2·3	−0·4	+0·7	0·3	0·06	4·8	33·7	35·9	16·1	85·7
W. Matthews, Coventry	96966	S.r., g.b., s.o., "Karrusel"	−0·3	−0·0	−0·5	−0·3	+0·7	0·5	0·05	4·5	30·4	38·5	16·7	85·6
J. White & Son, Coventry	34072	D.r.,g.b., s.o.	+0·3	−0·8	−0·5	−0·8	+2·1	0·4	0·04	7·0	32·0	36·4	17·1	85·5
S. Smith & Son, London	229-309 / 25574	S.r., g.b., s.o., non-magnetic "Karrusel"	+5·4	+5·1	+4·3	+7·2	+3·7	0·4	0·01	5·5	29·6	36·3	19·5	85·4
W. Matthews, Coventry	97747	S.r., g.b., s.o., "Karrusel"	−1·8	−1·1	−1·4	+0·7	−1·8	0·4	0·05	4·0	31·3	37·2	16·7	85·2
J. White & Son, Coventry	36645	S.r., g.b., s.o.	+0·9	+2·5	+1·5	+1·3	−0·5	0·3	0·09	6·7	34·2	36·9	13·9	85·0
Bonniksen, Coventry	55167	S.r., g.b., s.o., "Karrusel"	−3·6	−3·8	−4·5	−4·5	−5·1	0·5	0·06	4·5	30·7	38·0	16·3	85·0
Montandon-Robert, Geneva	1099	D.r.,g.b., s.o., chronograph	+0·5	−1·1	−0·5	+0·9	+2·4	0·3	0·06	5·2	33·0	36·0	15·9	84·9
Fridlander, Coventry	25577	S.r., g.b., s.o., "Karrusel"	−4·9	−5·3	−6·3	−4·0	−6·5	0·3	0·08	3·5	33·2	36·8	14·7	84·7
Newsome & Co., Coventry	131067	S.r., g.b., s.o., "Karrusel"	+3·6	+4·0	+4·8	+4·8	+3·4	0·6	0·02	3·2	28·5	37·8	18·4	84·7
Chas. J. Hill, Coventry	150188	S.r., g.b., s.o., "Karrusel"	+0·8	+0·5	+0·7	+1·5	+5·0	0·4	0·02	6·0	31·3	34·6	18·7	84·6
Fridlander, Coventry	14467	S.r., g.b., s.o.	+1·6	+2·4	+2·2	+1·9	+5·0	0·4	0·07	6·7	33·0	36·1	15·3	84·4
Matthews, Coventry	97263	S.r., g.b., s.o., "Karrusel"	+0·4	+1·6	+0·4	+3·6	+2·8	0·4	0·04	5·3	31·3	35·4	17·4	84·1

Table I—*continued.*

Watch deposited by	Number of watch.	Escapement, balance spring, &c.	Mean daily rate. Pendant up.	Mean daily rate. Pendant right.	Mean daily rate. Pendant left.	Mean daily rate. Dial up.	Mean daily rate. Dial down.	Mean variation of daily rate. ±	Mean change of rate for 1° F.	Difference between extreme gaining and losing rates.	Marks awarded for Daily variation of rate. 0—40	Marks awarded for Change of rate with change of position. 0—40	Marks awarded for Temperature compensation. 0—20	Total Marks. 0—100.
			secs.	secs.	secs.	secs.	secs.	secs.	secs.	secs.				
Fridlander, Coventry	25583	S.r., g.b., s.o., "Karrusel"	+0·1	−0·3	+0·1	−0·2	+2·6	0·4	0·07	7·5	31·7	36·6	15·6	83·9
Matthews, Coventry	36738	S.r., g.b., s.o., "Karrusel"	−0·2	−0·4	−0·2	−0·2	−0·6	0·6	0·07	4·7	28·3	39·4	16·1	83·8
" "	36742	S.r., g.b., s.o., "Karrusel"	+4·0	+1·4	+1·0	−0·1	+1·8	0·5	0·05	11·2	30·7	35·9	16·9	83·5
J. White & Son, Coventry	35382	S.r., g.b., s.o., "Karrusel"	−2·6	−1·8	−1·4	−1·8	−0·6	0·5	0·06	5·8	29·3	37·9	16·3	83·5
Fridlander, Coventry	25511	S.r., g.b., s.o., "Karrusel"	+0·5	+0·5	−0·0	−2·7	+1·4	0·3	0·09	8·0	33·2	35·8	14·3	83·3
S. Smith & Son, London	192 B10	S.r., g.b., s.o., "Karrusel"	−0·3	+0·3	−0·1	+1·1	+0·8	0·4	0·09	6·5	31·3	38·1	13·9	83·3
Fridlander, Coventry	25580	S.r., g.b., s.o., "Karrusel"	+0·3	−0·2	+0·3	+3·5	+3·8	0·3	0·05	5·7	33·4	33·3	16·5	83·2
A. Taylor, London	7558	D.r., g.b., s.o., annular "Tourbillon"	+1·2	+1·8	+1·9	+1·3	+1·7	0·4	0·11	6·2	31·8	38·9	12·5	83·2
J. White & Son, Coventry	33426	S.r., g.b., s.o.	+0·1	−2·5	−1·1	−0·4	−0·6	0·5	0·06	4·7	30·3	37·0	15·9	83·2
Usher & Cole, London	29932	S.r., g.b., s.o.	+2·0	+3·4	+2·7	+2·1	+2·6	0·5	0·06	6·7	29·1	38·3	15·7	83·1
S. Smith & Son, London	229–308 / 25578	S.r., g.b., s.o., "Karrusel," non-magnetic	+5·4	+4·8	+5·0	+3·6	+2·0	0·6	0·01	5·7	27·9	35·7	19·5	83·1
W. Matthews, Coventry	36688	S.r., g.b., s.o., "Karrusel"	+1·2	+0·9	+1·3	+2·3	+3·9	0·5	0·05	4·8	30·3	36·2	16·5	83·0
R. Waddington, Coventry	22186	S.r., g.b., s.o., "Karrusel"	+3·2	+3·7	+3·2	+2·6	+2·8	0·6	0·06	5·5	28·3	38·7	15·9	82·9
Montandon-Robert, Geneva	1072	D.r., g.b., s.o.	−1·1	+0·7	+0·9	+3·0	+3·4	0·4	0·06	6·2	32·7	34·2	15·9	82·8
S. Smith & Son, London	192 A2	S.r., g.b., s.o., "Karrusel"	+2·0	+3·0	+1·5	+0·2	+1·5	0·6	0·03	5·0	27·3	37·2	18·3	82·8
Matthews, Coventry	36326	S.r., g.b., s.o.	−0·7	−1·6	−1·3	+2·0	−1·6	0·6	0·03	5·8	28·9	35·8	17·9	82·6
Rotherhams, Coventry	23608	S.r., g.b., s.o.	+0·8	−0·5	−0·5	+1·8	+2·8	0·6	0·02	6·7	28·7	35·4	18·5	82·6
Stauffer, Son, & Co., London	169340	D.r., g.b., s.o., chronograph	+5·8	+3·4	+1·6	+4·1	+4·4	0·5	0·04	6·8	29·5	35·7	17·3	82·5
John Hewitt, Coventry	56179	S.r., g.b., s.o., "Karrusel"	+1·1	+1·3	+0·8	+0·5	+2·2	0·6	0·07	6·8	29·0	38·2	15·2	82·4
J. White & Son, Coventry	35765	D.r., g.b., s.o.	−1·3	−2·8	−0·6	+0·6	+1·8	0·4	0·06	6·2	31·6	34·5	16·2	82·3
Ehrhardt, Ltd., Birmingham	284583	S.r., g.b., s.o.	+2·0	+3·8	+4·4	+5·3	+5·9	0·5	0·05	5·5	29·8	35·6	16·9	82·3
J. White & Son, Coventry	35888	D.r., g.b., s.o.	+2·3	+1·6	−1·4	+1·8	+2·1	0·4	0·08	5·5	31·6	35·7	14·9	82·2
" " "	35841	S.r., g.b., s.o., "Karrusel"	+5·9	+5·2	+5·2	+6·2	+9·5	0·4	0·07	8·5	31·5	35·1	15·6	82·2
Baume & Co., London	254188	D.r., g.b., s.o., chronograph	+0·2	+1·2	−4·3	−1·4	−0·5	0·5	0·03	7·0	30·4	33·8	17·8	82·0
Johannsen & Co., London	6514	S.r., g.b., s.o.	−0·1	−0·1	−0·9	+1·8	−0·1	0·6	0·06	6·0	28·7	37·4	15·8	81·9

In the above List, the following abbreviations are used, viz.:—s.r. for single roller; d.r. for double roller; g.b. for going barrel; s.o. for single overcoil; + for gaining rate; − for losing rate.

Table II.

Highest Marks obtained by Complicated Watches during the year.

Description of watch.	Number.	Deposited by	Marks awarded for Variation.	Position.	Temperature.	Total marks.
			0—40	0—40	0—20	0—100.
Minute and split seconds chronograph, repeater, and perpetual calendar, with phases of the moon	148–99	S. Smith and Son, London	25·9	29·6	14·9	70·4
" " " " (and clock watch)	13159	" "	17·6	30·3	17·3	65·2
Minute chronograph and minute repeater	2324	J. W. Benson, London	25·5	34·0	11·5	71·0
" " "	153–4	S. Smith and Son, London	29·9	28·1	12·5	70·5
" " "	153–5	" "	24·5	27·1	15·9	67·5
Minute and split seconds chronograph	1099	Montandon-Robert, Geneva	33·0	36·0	15·9	84·9
" " "	169340	Stauffer, Son, and Co., London	29·5	35·7	17·3	82·5
" " "	12609	Baume and Co., London	30·4	33·5	15·8	79·7
" " "	3483	Wales and McCulloch, London	29·4	35·4	14·5	79·3
" " "	36352	J. White and Son, Coventry	28·3	32·8	17·9	79·0
Minute and seconds chronograph	254188	Baume and Co., London	30·4	33·8	17·8	82·0
" " "	21633	Rotherhams, Coventry	28·1	35·1	16·4	79·6
" " "	29975	J. Player and Son, Coventry	27·9	38·3	12·5	78·7
" " "	6546	Army & Navy C. S., London	29·1	35·9	13·6	78·6

Table II—*continued.*

Description of watch.	Number.	Deposited by	Marks awarded for Variation.	Position.	Temperature.	Total marks.
			0—40	0—40	0—20	0–100.
Minute repeater	1095	Montandon-Robert, Geneva	32·6	37·3	16·1	86·0
" "	3448	Wales and McCulloch, London	28·5	33·9	15·2	77·6
" " (5 minute)	5240	Army & Navy C. S., London	29·8	34·6	12·0	76·4
"Non-magnetic"	229–309 / 25574	S. Smith and Son, London	29·6	36·3	19·5	85·4
"	229–308 / 25578	" "	27·9	35·7	19·5	83·1
"	25514	Fridlander, Coventry	32·0	34·2	14·7	80·9
"	192 C 225	S. Smith and Son, London	30·4	34·3	15·6	80·3

May 10, 1900.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read, as follows:—

Burch, George James, M.A.
David, Professor T. W. Edgeworth, B.A.
Farmer, Professor John Bretland, M.A.
Hill, Leonard, M.B.
Horne, John, F.G.S.
Lister, Joseph Jackson, M.A.
MacGregor, Professor James Gordon, D.Sc.
Manson, Patrick, M.D.
Muir, Thomas, M.A.
Rambaut, Professor Arthur A., M.A.
Sell, William James, M.A.
Spencer, Professor W. Baldwin, B.A.
Walker, Professor James, D.Sc.
Watts, Philip.
Wilson, Charles T. R., M.A.

The following Papers were read:—

I. "On the Diffusion of Gold in Solid Lead at the Ordinary Temperature." By Sir W. C. ROBERTS-AUSTEN, K.C.B., F.R.S.

II. "On Certain Properties of the Alloys of the Copper and Gold Series." By Sir W. C. ROBERTS-AUSTEN, K.C.B., F.R.S., and T. KIRKE ROSE, D.Sc.

III. "Experiments on the Value of Vascular and Visceral Factors for the Genesis of Emotion." By Professor C. S. SHERRINGTON, F.R.S.

IV. "On the Brightness of the Corona of April 16, 1893. Preliminary Note." By Professor H. H. TURNER, F.R.S.

V. "Radio-activity of Uranium." By Sir W. CROOKES, F.R.S.

May 17, 1900.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "The Circulation of the Surface Waters of the North Atlan Ocean." By H. N. DICKSON. Communicated by Sir JO MURRAY, F.R.S.

II. "On Cerebral Anæmia and the Effects which follow Ligation the Cerebral Arteries." By Dr. LEONARD HILL. Commu cated by Dr. MOTT, F.R.S.

III. "The Influence of Increased Atmospheric Pressure on the Circu tion of the Blood. Preliminary Note." By Dr. LEONA HILL. Communicated by Dr. MOTT, F.R.S.

IV. "Contributions to the Comparative Anatomy of the Mammali Eye, chiefly based on Ophthalmoscopic Examination." By I G. L. JOHNSON. Communicated by Dr. GADOW, F.R.S.

The Society adjourned over Ascension Day to Thursday, May 31.

"Electrical Conductivity in Gases traversed by Cathode Rays By J. C. MCLENNAN, Demonstrator in Physics, University Toronto. Communicated by Professor J. J. THOMSON, F.R. Received December 7, 1899,—Read February 1, 1900.

(Abstract.)

The object of the experiments which are described in this pap was to investigate the nature of the conductivity produced in differe gases when cathode rays of definite strength passed through them.

In a series of papers,* Professors J. J. Thomson and Rutherfo have recently shown that gases become conductors, when travers either by Röntgen or by uranium rays, owing to the production positive and negative ions throughout their volume.

In the present investigation cathode rays were found to impress condition of the same kind upon a gas, and laws have been d veloped which connect the absorption of these rays with the numb of ions produced by them in the absorbing gases.

The investigation is described under the following subdivisions :—

(1) Form of tube adopted for the production of cathode rays.
(2) Ionisation by cathode rays.
(3) Discharging action of cathode rays.
(4) Ionisation not due to Röntgen rays.

* 'Phil. Mag.,' November, 1896, p. 393; ibid., January, 1899, p. 109.

2 F 2

(5) Discussion of methods for measuring the ionisations produced in different gases.
(6) Description of apparatus used.
(7) Explanation of the method adopted for comparing ionisations.
(8) Ionisation in different gases at the same pressure.
(9) Ionisation in air at different pressures.
(10) Ionisation in a gas independent of its chemical composition.
(11) Comparison of ionisations produced by cathode and by Röntgen rays.
(12) Summary of results.

The tube used for the production of cathode rays was similar in form to that devised by Lenard,* but, as the brass plate carrying the aluminium window was found to act very well as an anode, the ordinary positive electrode in his apparatus was dispensed with.

The paper commences with a series of experiments illustrating the conductivity produced by cathode rays, and the various phenomena met with are shown to be fully explained on the supposition that positive and negative ions are produced in a gas by the radiation, and that the conductivity arises from the motion of these ions under the action of an electric force.

This view of the conductivity is also shown to explain the loss of charge sustained by a conductor upon which the rays fall. Lenard's experiments† in this connection were repeated, and, contrary to his observations, negative charges were not in any case found to be completely dissipated by the rays, but were reduced, at atmospheric pressure, to small limiting values of the order of 0·25 volt. These values were found to be slightly increased when a blast was directed so as to remove the air close to the conductor, and when the latter was placed in a vacuum, the limiting charge rapidly assumed a very high value. The value of the limiting charge was found to be affected also by the proximity of conductors whose potentials were different to that of the one upon which the rays fell.

Conductors initially unelectrified gained the limiting negative charge under the action of the rays, and positive electrifications upon conductors, surrounded by air at normal pressure, were completely discharged.

The explanation offered regarding this limiting or steady state is that it represents a condition of equilibrium in which the electric convection by the rays to the conductor is just equal to the conduction by the ionised gas away from it.

It has been thought by some that the ionisation under consideration may be due to Röntgen rays sent out from the aluminium window at

* 'Wied. Ann.,' vol. 51, 1894, p. 225.
† 'Wied. Ann.,' vol. 63, 1897, p. 253.

the same time as the cathode rays. The results of experiment, however, are entirely opposed to this view, and lead to the conclusion that, if any Röntgen rays are present in the cathode pencil, they must be of so weak a character that their ionising action can be neglected. A direct comparison showed the ionisation by cathode rays to be about 300 times that due to an intense Röntgen radiation.

In the conductivity produced by cathode rays, the current of electricity does not increase in proportion to the electromotive force applied. The current, after reaching a certain critical value, becomes practically stationary and increases but little when very large increases are made in the electric field. With Röntgen or uranium radiation fields of 400 or 500 volts a centimetre have sufficed to give saturation in the case of most simple gases, but in the present investigation it was necessary to go as high as 1000 volts a centimetre before the maximum current was reached.

In order to compare the ionisations in two different gases, or in the same gas under different conditions, recourse was had to the use of two ionising chambers. The discharge tube was provided with a double cathode, and carried two aluminium windows. Two pencils of rays were obtained in this way, whose intensities were found to maintain a constant ratio, and these were used to produce the ionisations in the two chambers.

The ionisation in air, kept at a constant pressure in one of the chambers, was taken as the standard. The gases, whose ionisations were to be compared, were placed in turn in the other chamber, and their conductivities, as measured by saturation currents, were found in terms of the standard.

An important result, obtained by this method with cathode rays of constant intensity, was the agreement found to exist between the ionisation in hydrogen at atmospheric pressure, and that in air at a pressure of 53 mm. At these pressures the two gases had the same density, and in both cases, therefore, according to Lenard's absorption law, the disposition of the rays, their actual intensities, and the amount of them absorbed from point to point in the ionising chamber, were precisely the same. Under these conditions the equal ionisations obtained not only form a strong confirmation of Lenard's absorption law, but they also show that, where equal absorption of cathode rays occurs, equal ionisation is produced.

In order to test the conclusion more closely experiments were made with air, hydrogen, carbon dioxide, oxygen, nitrogen, and nitrous oxide, and in all cases it was found that, when these gases were reduced to the same density, the same ionisation was produced in them by rays of constant intensity.

It follows, therefore, that an ionisation law exists exactly analogous to that of absorption, namely, that when cathode rays of a given

strength pass through a gas, the number of ions produced per second in 1 c.c. depends only upon the density of the gas, and is independent of its chemical composition.

From the results thus obtained, the conclusion is drawn that, when cathode rays are absorbed to any extent, the positive and negative ions produced by these absorbed rays are of a definite amount, which bears a constant ratio to the quantity of the rays absorbed; that is to say, in order to ascertain the relative ionisations produced in any two gases by cathode rays of the same intensity, it is sufficient to determine the absorbing powers of the two gases for the same rays. In other words, the coefficients of ionisation are determined when the coefficients of absorption for the same gases are known.

The paper then deals with the ionisation in any particular gas under varying pressures. The inference is drawn that, under rays of constant intensity, the ionisation in a particular gas varies directly with the pressure. The very great absorption of the rays by gases at ordinary pressures prevented a direct verification of this relation; but, as Lenard has shown, the coefficients of absorption for any particular gas to vary directly with the pressure, the conclusion seems quite justifiable in the light of the connection established between ionisation and absorption.

Assuming this relation to be true, it follows at once that, if rays of constant intensity are allowed to traverse different gases at the same pressure, the ionisations produced would be directly proportional to the densities of these gases.

These numbers for the gases examined are given in column 1 of the appended table, while in column 2 are given the values found by Professor J. J. Thomson for the relative ionisation produced in these same gases by Röntgen rays of constant intensity.

Gases examined.	Column I. Ionisation by cathode rays (calculated).	Column II. Ionisation by Röntgen rays (observed).
Air	1·00	1·00
Oxygen	1·106	1·10
Nitrogen	0·97	0·89
Carbon dioxide	1·53	1·40
Hydrogen	0·069	0·33
Nitrous oxide..........	1·52	1·47

The numbers, with the exception of those for hydrogen, present a fair agreement, and they show that although the two forms of radiation are so very different in many respects, still the products of their actions on the gases examined are practically the same.

"Observations on the Electromotive Phenomena of Non-medullated* Nerve." By Miss S. C. M. SOWTON. Communicated by Dr. WALLER, F.R.S. Received March 8,—Read March 29, 1900.

[PLATE 4.]

Kühne and Steiner in their work on the olfactory nerve of the pike,† published in 1880, showed that the laws of electromotive action in medullated nerve, as formulated by Du-Bois Reymond, held good in the case of non-medullated fibres. Having demonstrated the resting current in a suitably prepared olfactory nerve, they proceeded to compare the value of this current with that obtained from medullated nerves of the same fish as well as from the sciatic nerve of the frog. The result showed a much higher E.M.F. for non-medullated than for medullated nerves, their diameter being about equal; a result confirmed by Biedermann and others working on the non-medullated nerves of Anodonta.

Kühne and Steiner found that the resting current diminished rapidly, but a new transverse section restored it to, and in some cases even augmented, its original E.M.F.

On stimulation with induction currents, a negative variation of high value was obtained. The response was unfailing while the nerve was fresh, provided that the stimulating electrodes were not applied too near the peripheral end of the nerve where its character becomes modified.

While working at Leipzig during the early part of 1899, Professor Hering was good enough to suggest that I should use the nerve which had given such good results in the hands of Kühne and Steiner for a further study of electromotive phenomena in non-medullated nerve, and especialty with reference to the occurrence in such fibres of the "positive after-variation" which his own researches‡ and those of Head§ had made familiar in the case of medullated nerve. Hering had himself occasionally observed this effect in 1885, while using the olfactory nerve for experiments on electrotonus. The positive after-effect occurred most frequently on mechanical stimulation of the nerve by cutting, more rarely it followed when induction currents were used.

In the present experiments, the pike used ranged from $1\frac{1}{2}$ to $2\frac{1}{3}$ kilos.

* "Non-medullated" is used in its ordinary sense as applied to grey nerve, and without prejudice to the conclusions of Gad and Heymans that such nerves may be slightly myelinated, conclusions which Ambronn and Held have confirmed by means of their "optical method." 'Archiv für Anat. und Physiol.,' 1896, p. 210.

† K. und S. 'Untersuchungen des Physiol. Instituts der Universität Heidelberg,' Bd. 3, p. 149.

‡ 'W. S. B.,' vol. 89, 3 Abth., p. 137.

§ 'Pflüger's Arch.,' vol. 40, p. 207.

in weight. As described by Kühne and Steiner the E.M.F. of the resting current of the olfactory nerve was found to be very high, and a few experiments, made by the "opposition method" of those authors, gave results similar to theirs; that is to say, the resting current of the non-medullated nerve overpowered that of a medullated nerve of approximately similar thickness, whether taken from the fish itself or from a frog.

The galvanometer used was a sensitive and quickly reacting instrument by Siemens and Halske, of Berlin, a modified form of the D'Arsonval pattern. It was used with a telescope in the usual way.

Method.—To prepare the olfactory nerves: the head of the pike was cut off, the lower jaw removed, and the head fixed to a small board. The bones of the upper surface of the skull, from the brain down to the nostrils, were removed with bone forceps, care being taken that only bone was cut away, the cartilage below being left uninjured. The cartilage thus exposed forms a thin capsule over the lobes of the brain, becoming thicker towards the olfactory lobes, and forming, where these are prolonged into the olfactory nerves, a substantial sheath which encloses the nerves entirely, and through the semitransparent walls of which the nerves are just visible. To expose the nerves a fine sharp scalpel was used, with which the upper surface of the cartilaginous sheath was sliced away, great care being taken not to cut too deeply and so injure the nerves themselves. Having removed in this way their upper covering, the two grey nerves are found lying side by side in their canal: they run parallel for the greater part of their length, then fork, right and left, to enter either nostril. Before attempting to remove the nerves, it must be carefully ascertained that they are freely exposed in their entire length, with no overhanging shreds of cartilage to catch and injure them. The way being clear, the end-organ of each nerve is separated from its nostril and serves as a handle by which the nerve is lifted from its canal; a clean scissor cut then severs its central end from the olfactory lobe, and the nerve is ready for experiment* (see Plate).

The central end of the nerve was led off from transverse and longitudinal sections by brush electrodes; their distance apart was usually 5 mm. The stimulating electrodes were of platinum wire, with Hering's† extra loop to cut off unipolar effects. The induction coil was supplied by a single Daniell cell, the distance between primary and secondary coils varying from 6 to 0 c.m.

A good olfactory nerve, freshly prepared, gave in response to single stimuli, electrical or mechanical, a negative variation that was perfectly legible on the galvanometer scale, but it must be noted that

* If, as sometimes happened, the two nerves were united near their central end they were used together as one nerve.

† *Described* by Pereles and Sachs, 'Pflüger's Arch.,' vol. 52, p. 529.

only while the nerve was quite fresh could such responses be obtained by *single* stimuli. The momentary mechanical stimulus consisted in a clean cut through the nerve with fine sharp scissors, the blades of which had been moistened with normal saline; the nerve was supported, so that there should be no pull upon the led-off portion.

In the tables of experiments given below, a few cases will be noticed in which there is a back swing + beyond the position of rest. This is partly instrumental swing, but partly also due to a slight positive after-effect. The galvanometer used, although highly damped, was not perfectly dead-beat, and the small after-effects could not be accurately estimated. All that can be said is that there is a slight tendency towards positive after-effects on good nerves when quite fresh; later the back swing tends to fall short of the position of rest.

With tetanising induction currents the positive after-effect was only once observed with any certainty.

Olfactory Nerve of Pike.—Nerve current compensated. The nerve current is +, the negative variation −, on the scale throughout the experiments.

Exp. 1.—March 8, 1899. Pike, weighing 5½ lbs. Nerve I. Stimulation by single break induction shocks.

Coil distance.	Position of rest.	Negative variation.	Back swing.	Value of neg. var.	Back swing + or −*.	
0 cm.	71	69·5	71·7	−1·5	+0·7	..
"	63	61·5	63·2	−1·5	+0·2	..
"	54	52·7	54·4	−1·5	+0·4	..
0 cm.	47	45·6	46·7	−1·4	..	−0·3
"	41	39·7	40·7	−1·4	..	−0·3
"	45	43·7	44·5	−1·3	..	−0·5
Nerve moistened. Stimulation by single make induction shocks.						
0 cm.	49	48	49·6	−1	+0·6	..
"	47	45·9	46·2	−1·1	..	−0·8
Same nerve. Ten stimuli make and break alternately at half-second intervals.						
0 cm.	38·5	27·7	36·8	−10·8	..	−1·7
"	49	38·6	47	−10·4	..	−2

* In this column the + sign signifies a back swing beyond the position of rest, the − sign signifies that the back swing fell short of that position.

Exp. 2.—Nerve 2. Stimulation by single break induction shocks.

Coil distance.	Position of rest.	Negative variation.	Back swing.	Value of neg. var.	Back swing + or −.	
5 cm.	52·5	50·7	52·5	−1·8	..	..
"	49	47·2	48·8	−1·8	..	−0·2
0 cm.	45	43·4	44·9	−1·9	..	−0·1
Stimulation by ten shocks, alternate make and break, at half-second intervals.						
0 cm.	41	26·2	41·5	−14·8	+0·5	..
"	47·5	38·4	47·4	− 9·1	..	−0·1

Exp. 3.—February 21, 1899. Pike, 4¼ lbs. Nerve II. Momentary mechanical stimulation by cutting.

Position of rest.	Negative variation.	Back swing.	Value of neg. var.	Back swing + or −.	
88·5	86·8	87	−1·7	..	−1·5
86·6	85·2	very slow return	−1·4	..	..

Exp. 4.—March 6. Pike, 4 lbs. Nerve II. Stimulation by cutting.

48	44·5	50	−3·5	+2	..
40	36·2	41	−3·8	+1	..

Exp. 5.—March 8. Pike, 5½ lbs. Nerve II. Stimulation by cutting.

48	46	49	−2	+1	..

Through the kind co-operation of Dr. Siegfried Garten, Assistant in the Leipzig Laboratory, I was enabled to make observations with the capillary electrometer on the response of the olfactory nerve to single stimuli. The photographic records of three such observations are given below. The curves as reproduced are rather less than half their original size; they read from left to right. The negative variation appears as an upward movement of the mercury meniscus.

Fig. 1.

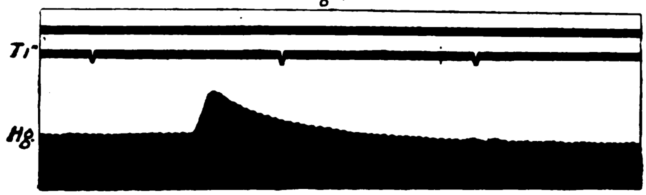


Fig. 2.

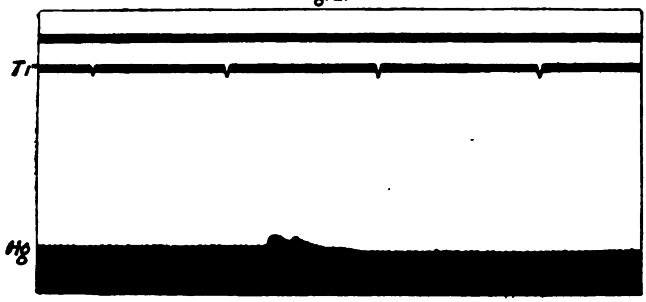


Fig. 3.

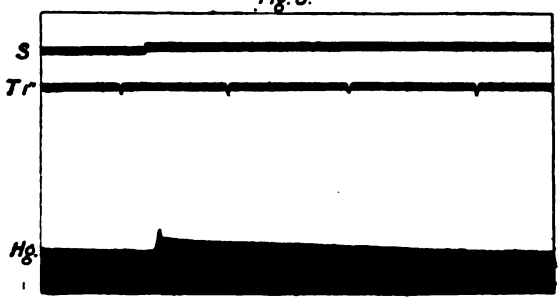


Fig. 1.—March 23, 1899. Pike of $2\frac{7}{10}$ lbs. weight. Olfactory nerve I quite fresh Momentary mechanical stimulation by scissor-cut. Following upon the nega tive variation there is a very slight and slowly developed positive after-effect.

Fig. 2.—March 20.—Pike of $5\frac{1}{4}$ lbs. The two olfactory nerves are used togethe quite fresh. Mechanical stimulation by cutting. The negative variation i seen as two responses incompletely fused, obviously owing to the fact that th stroke of the scissors affected the two nerves successively.

Fig. 3.—March 18. Olfactory Nerve I. Stimulation by single break inductio shock coil O.C.M. The signal S shows the moment of stimulation. In al three curves the time is marked in seconds.

In a further series of observations the nerve was stimulated a regular intervals by brief tetanising currents, the galvanometric deflec tions being sometimes photographically recorded (Waller's method*)

* Croonian Lecture, 'Phil. Trans.,' B, 1897.

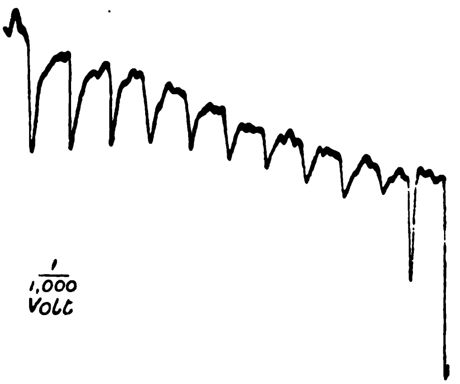
These experiments brought out very evidently what I am disposed to regard as a principal difference between medullated and non-medullated nerve. Whereas in medullated nerve successive effects diminish very slightly or not at all, or actually increase ("staircase effect"), non-medullated nerve always exhibits a comparatively rapid decrease of successive effects.

Figs. 4 and 5 illustrate this point.

FIG. 4.

Medullated nerve (frog). Stimulation at 1-minute intervals.

FIG. 5.



Non-medullated nerve (pike). Stimulation at 1-minute intervals.

The galvanometer used in these experiments was a Thomson, of Muirhead's pattern, and was dead-beat. It was used with or without photographic apparatus, the spot of light being in the last case reflected on to a large transparent screen in the half-darkened room.

The highly damped Thomson reacted too slowly for such fleeting effects as the response of the nerve to single stimuli to be made visible, but being dead-beat was most suitable for the study of positive after-effect.

With a normal olfactory nerve in its fresh state the positive after-effect was never observed with this instrument, but a most striking development of the phenomenon followed on subjecting the nerve to a *stream* of carbon dioxide. The nerve was enclosed in a small moist

chamber with inlet and outlet tubes, and was led off in the usual way. The stimulation by platinum wires was given at one minute intervals. After a few normal responses—the character of which was a large negative deflection often with incomplete return to the position of rest—carbon dioxide was driven into the nerve chamber; its immediate effect was a marked diminution of the negative variation and the appearance of a positive after-variation, which increased in size for several minutes, attaining often as much as three times the size of the negative variation. At this point of highest development of the positive after-effect, the back swing was very rapid, and began before the close of stimulation. The positive after-effect then began to pass off, and a few minutes later the deflection was once more purely negative. Larger doses of carbon dioxide produced anæsthesia of the nerve, but with returning excitability the positive after-effect appeared as above described.

At the period when the distinct positive after-effect had passed off a curious phase was sometimes noticeable immediately after stimulation, viz., a quick to-and-fro movement, suggesting the struggle of two opposing processes before the slow return of the spot of light towards its position of rest. For instance, in the last line of figures in Experiment 6 the spot stands at 19·5; on stimulation it moves − to 9·5, then after the stimulus + to 11, − to 10, and slowly back to 18.

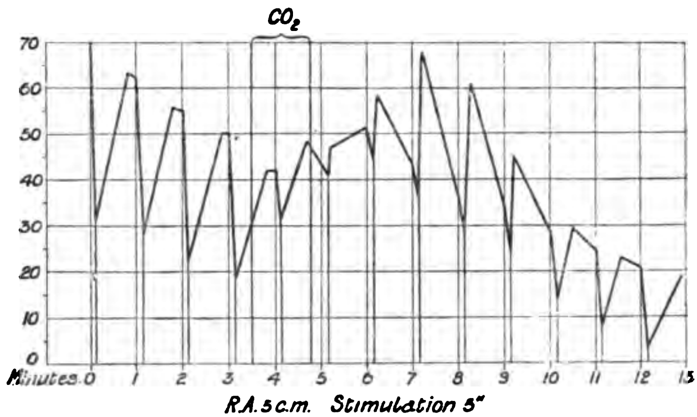
April 13, 1899. Olfactory Nerve I. Stimulation by Tetanising Currents. Nerve Current off Scale Spot, brought back by Magnet.

Stim.	Coil.	Zero.	Neg. var.	Back swing.	Value of neg. var.	Back swing. + or	Back swing. Incomplete.
5″	5 cm.	70	33	63	−37	—	Less 7
At 1 min.	,,	62	29	56	−33	—	,, 6
intervals.	,,	55	23	50	−32	—	,, 5
	,,	50	18·5	42	−31·5	—	,, 8
			CO_2 1½ mins.				
	,,	42	31	48·5	−11	+6·5	
	,,	42·5	40·5	47·5	− 2	+ 5	
Exp. 6..	,,	51	45	59	− 6	+ 8	
	,,	43*	36	67·5	− 7	+24·5	
	,,	37*	28	61	− 9	+24	
	,,	34	24	45	−10	+11	
	,,	29	14	29·5	−15	+ 0·5	
	,,	25	8	23	−17	—	,, 2
	,,	21	4	19	−17	—	,, 3

* The + after effect began before the end of stimulation.

April 13, 1899. Olfactory Nerve I—*continued.*

Stim.	Coil.	Zero.	Neg. var.	Back swing.	Value of neg. var.	Back swing. + or	Back swing. Incomplete.
			Spot readjusted.				
	5 cm.	51	*30·5*†	47	−20·5	—	Less 4
	„	46	*26·5*,† *25·5*†	43	−21	—	„ 3·5
	„	43	24·5, 25, *23*†	40	−20	—	„ 3
	„	40	22, 22·5, 21	37	−19	—	„ 3
	„	37	20, 19, 18	35	−19	—	„ 2
	„	35	19, 19·5, 16	33	−19	—	„ 2
	„	33	16·5, 17, 14·5	30·5, 31	−18·5	—	„ 2
	„	31	15, 15·5, 13	—	−16	—	
			CO_2 2 mins.				
	„	28*	24	35	− 4	+ 7	
	„	31	30	31·5	− 1	+ 0·5	
	„	39	—	—	—	—	
	„	48	44·5	50·5	− 3·5	+ 2·5	
	„	34*	31	51	− 3	+ 17	
	„	31	26	45	− 5	+ 14	
	„	29	23	34·5	− 6	+ 15·5	
	„	26	19	27	− 7	+ 1	
	„	24	16, *20·5*†	25	− 8	+ 1	
	„	23·5	13, 16, 15·5	23	−10·5	—	„ 0·5
	„	22	11·5, *14*†	20·5	−10·5	—	„ 1·5
	„	19·5	9·5, 11, 10	18	−10	—	„ 1·5



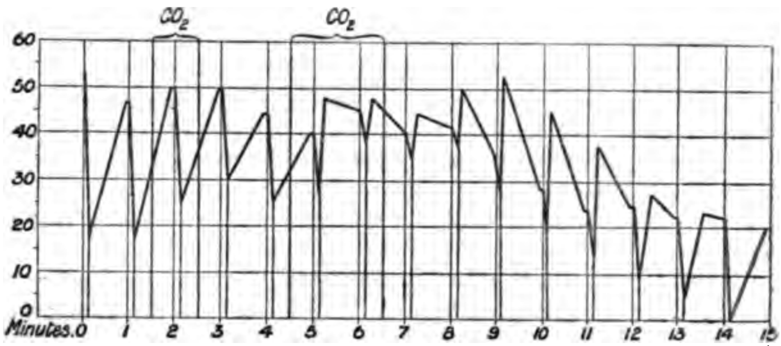
Curve of first part of Experiment 6.

* The + after-effect began before the end of stimulation.

† *A pause of a second or so took place here.*

April 14, 1899. Olfactory Nerve.

Stim.	Coil.	Zero.	Neg. var.	Back swing.	Value of neg. var.	Back swing. + or	Back swing. Incomplete.
5″	6 cm.	53	18·5	47	−34·5	—	Less 6
	,,	47	17	—	−30		
			CO_2 1 min. (slow stream).				
	,,	50	25	50	−25	—	
	,,	50	30	44	−20	—	,, 6
	,,	44	25·5	—	−18·5	—	
			CO_2 2 mins.				
Exp. 7..	,,	40·5	28	47·5	−12·5	+ 7	
	,,	45	38·5	48	− 6·5	+ 3	
	,,	41	35·5	44	− 5·5	+ 3	
	,,	41·5	37	50	− 4·5	+ 8·5	
	,,	35	27·5	52	− 7·5	+17	
	,,	28	19·5	45·5	− 8·5	+17·5	
	,,	24·5	14	37·5	−10·5	+13	
	,,	24	8·5	27	−15·5	+ 3	
	,,	22	5	23	−17	+ 1	
	,,	22·5	1	20·5	−21·5	—	,, 2
			Spot adjusted.				
	,,	50	23	46·5	−27	—	,, 3·5
	,,	46·5	22·5	—	−24	—	
			CO_2 2 mins.				
	,,	50	44	60	− 6	+10	
	,,	47·5	45	48	− 2·5	+ 0·5	
	,,	46	43·5	52·5	− 2·5	+ 6·5	
	,,	48	40	58	− 8	+10	
	,,	36	30	55	− 6	+19	
	,,	35	28·5	59·5	− 6·5	+24·5	

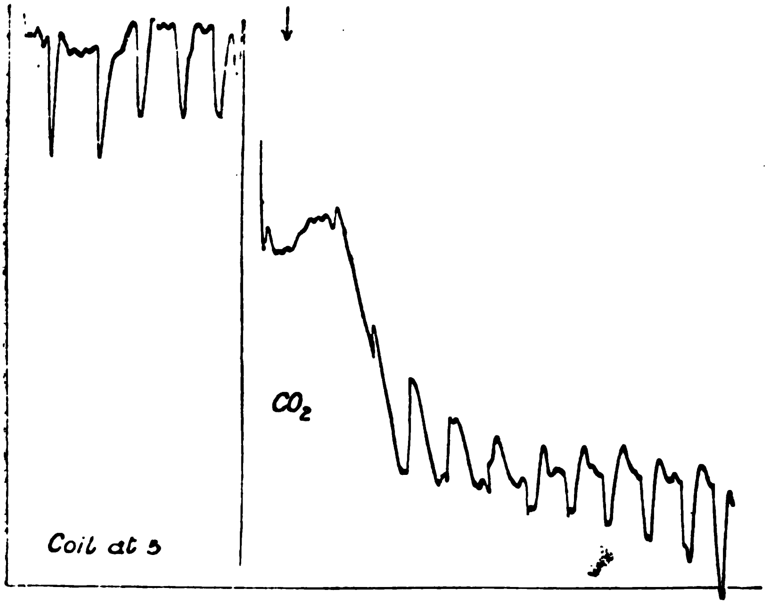


Coil 6 c.m Stimulation once per min. for 5 seconds.

Curve of first part of Experiment 7.

Fig. 6 is a photographic record of a similar experiment.

Fig. 6.



Effect of CO_2 on the negative variation of non-medullated nerve. Stimulation at 1-minute intervals.

Another condition in which the olfactory nerve gave positive after-effects was after being kept for some time in normal saline. Isolated grey nerve appears to be far less resistant than white nerve, but the olfactory sometimes retained its excitability for as long as four hours after excision, and the electrical response of such a kept nerve was usually a negative effect followed by a large positive after-effect. In frog nerve there is also a development of positive after-effect in stale nerve; but whereas such effects are in this case markedly reduced if not abolished by a new transverse section, in the case of the olfactory nerve the positive after-variation persists after fresh section.

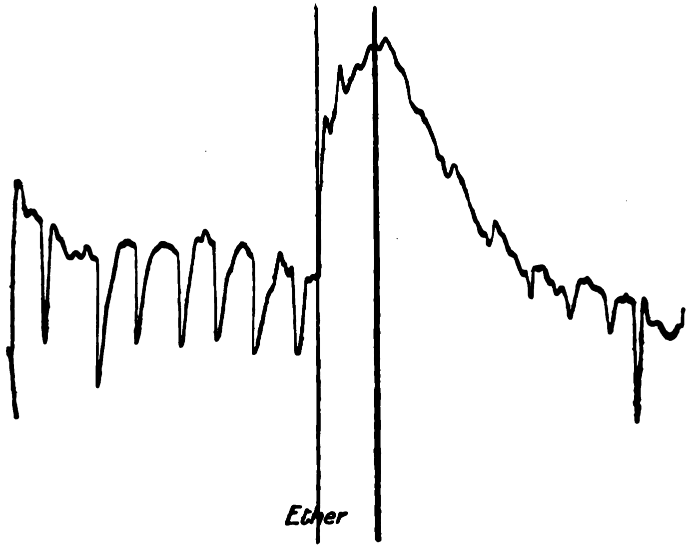
A few experiments were made to test the effect of ether and chloroform vapour on the negative variation of non-medullated nerve. The galvanometric effect was promptly abolished by brief administration of ether or chloroform vapour. Recovery after anæsthesia occurred to some extent—more markedly in the case of ether than in that of chloroform.

I wish to take this opportunity of thanking Professor Hering for the means of study which I enjoyed at Leipzig, and for his kindness in sparing me much of his own valuable time.

I would also thank Dr. Waller for kind help and suggestions.

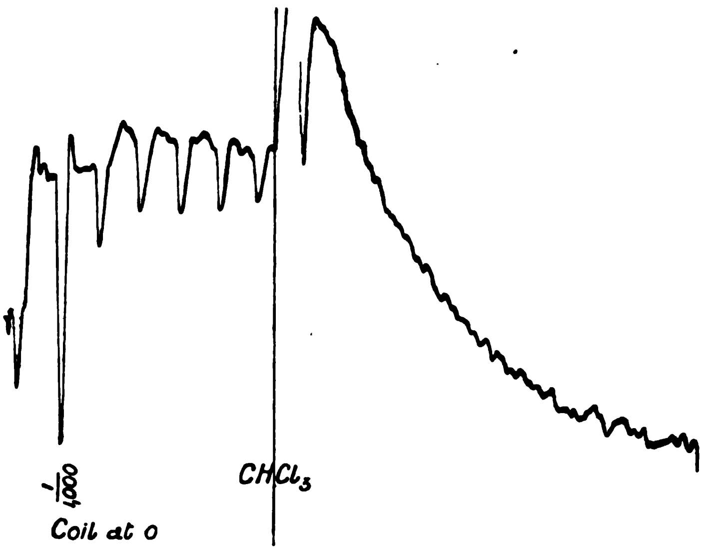
OLFACTORY NERVES OF PIKE PREPARED FOR EXPERIMENT.

Fig. 7.



Effect of ether on negative variation of non-medullated nerve (Pike).

Fig. 8.



Effect of chloroform on the negative variation of non-medullated nerve (Pike).

"Experiments on the Value of Vascular and Visceral Factors for the Genesis of Emotion." By C. S. SHERRINGTON, M.A., M.D., F.R.S. Received April 5,—Read May 10, 1900.

(From the Physiological Laboratory, University College, Liverpool.)

That marked reactions of those portions of the nervous system which regulate the activity of the thoracic and abdominal organs and the skin do contribute characteristically to the phenomena of emotion has long been common knowledge. In descriptions of emotion furnished in recent years by certain leading psychologists these purely physiological processes have been given a place more important than was attributed to them formerly. To changes induced in the condition of the heart and blood vessels, lungs, abdominal and pelvic viscera and skin has been assigned a large causal rôle in the genesis of affective psychological states. Whereas the cardiac, vascular, respiratory, and visceral phenomena accompanying emotion were wont to be regarded as secondary to the cerebral and psychological, we find their position reversed in the writings of Professor W. James,* Professor C. Lange,† and Professor Sergi.‡ It is true that it is claimed that this more recent position has been foreseen and partly preoccupied by older writers, by Descartes§ and Malebranche,‖; but as Professor Ribot, who with some reservation, endorses the new theory,¶ writes: "La supériorité de James et de Lange, c'est de l'avoir posée clairement et de s'être efforcés de l'appuyer sur des preuves experimentales."** It is scarcely fitting here to enter on a full statement of the doctrine. I may, however, be allowed some brief quotations from the authorities indicating their teaching.

After having, in a previous chapter, given an account of the influence that a shock of feeling exerts on the nerve centres controlling circulation, respiration, skin glands, abdominal and pelvic viscera, Professor James writes: "Our natural way of thinking about these coarser emotions (*e.g.*, 'grief, fear, rage, love') is that the mental perception of some fact excites the mental affection called the emotion, and that this latter state of mind gives rise to the bodily expression.

* 'Mind,' London, 1884, 'Principles of Psychology,' London, 1890, vol. 2, pp. 448, &c.

† Om Sindsbevägelser, Copenhagen, 1885; German by Kurella, Leipzig, 1887; French by Georges Dumas, Paris, 1895.

‡ Dolore e Piacere, Milano, 1894, 398 pp. 'Zeitschft. f. Psychologie u. d. Physiol. der Sinnesorgane,' Hamburg and Leipzig, April, 1897, p. 96, &c.

§ 'Passions de l'âme,' Paris, 1648–9; 'Passiones sive Affectus Animae,' Amstelod. 1677.

‖ 'Recherche de la Verité,' 1672.

¶ 'La Psychologie des Sentiments,' p. 92—113. Paris, 1896.

** *Ibid.*, p. 112.

My theory on the contrary is that *the bodily changes follow directly the perception of the exciting fact, and that our feeling of the same changes as they occur* IS *the emotion.*"* "*Every one of the bodily changes, whatsoever it be, is* FELT, *acutely or obscurely, the moment it occurs.* If the reader has never paid attention to this matter, he will be both interested and astonished to learn how many different local bodily feelings he can detect in himself as characteristic of his various emotional moods."† "If we fancy some strong emotion and then try to abstract from our consciousness of it all the feelings of its bodily symptoms we find we have nothing left behind, no 'mindstuff' out of which the emotion can be constituted, and that a cold and neutral state of intellectual perception is all that remains."‡ "If I were to become corporeally anæsthetic, I should be excluded from the life of the affections, harsh and tender alike, and drag out an existence of merely cognitive or intellectual form."§

This view is the extreme antithesis to the spiritualistic conception of emotion. On it the "coarser emotions" come to consist in essence merely of sensations which arise in consequence of the effect of an idea upon the internal organs. M. Jules Soury says,|| "Pour James l'emotion n'est que la conscience que nous avons des réactions organiques, vasculaires, glandulaires, matrices, &c., provoqués par certaines perceptions ou certains souvenirs."

Professor Lange traces the whole psycho-physiology of emotion to certain excitations of the vasomotor centre. He conceives all the other of the organic reflexes occurrent in emotion to be attributable mediately to the vasomotor. For him, as for Professor James, the emotion is the outcome and not the cause or the concomitant of the organic reaction; but for him the foundation and corner-stone of the organic reaction is as to physiological quality vascular, namely, vasomotor. Emotion is an outcome of vasomotor reaction to stimuli of a particular kind. The stimulus is some sensation acting often by intermediation through some memorial-idea linked to it by association. This stimulus induces a vasomotor action in viscera, skin, and brain. The change thus induced in the circulatory condition of these organs induces changes in the actions of the organs themselves, and these latter changes evoke sensations which constitute the essential part of emotion. It is by excitation of the vasomotor centre therefore that the exciting cause, whatever it may chance to be, of emotion produces the organic phenomena which as felt constitute for Lange the whole essence of emotion. It is noteworthy that in Lange's view the action of the

* The italics and emphasis stand as in the original.

† 'Principles of Psychology,' vol. 2, p. 450, London, 1890.

‡ *Ibid.*, vol. 2, p. 451.

§ *Ibid.*, vol. 2, p. 452.

|| 'Du Système Nerveux,' Paris, 1899, vol. 2, p. 1388.

vasomotor centre upon the blood-vessels of the brain, as well as on those of the viscera and skin, plays an important part.

The teaching of Professor Sergi closely approximates to that of Lange. He argues that the exciting stimulus acts on nervous centres in the bulb (medulla oblongata) producing cardiac vascular and respiratory effects as well as effects upon the abdominal and pelvic viscera. He writes recently: "Lange hat gemeint, die Affekte hingen von dem vasomotorischen Zentrum ab; doch ist dieses Zentrum zu eng, um die Mannigfaltigkeit der visceralen Erscheinungen des Ernährungs lebens erklären zu können. Dagegen hat mich die Analyse zu der Erkenntniss gebracht dass der Bulbus rachidicus, wo die reflex und automatischen Zentren der Nerven, die das ganze Ernährungsleben regulieren, zusammenlaufen, das Zentrum der Affekte und im allgemeinen das der Gefühle ist."*

The views of James, Lange, and Sergi have common to them this, that according to them the psychological process of emotion is secondary to a discharge of nervous impulses into the vascular and visceral organs of the body suddenly excited by certain peculiar stimuli, and depends upon the reaction of those organs. Professor James's position in the matter is, however, not wholly like that of Professor Lange. In the first place, he does not consider vasomotor reaction to be primary to all the other organic and visceral disturbances that carry in their train the psychological appanage of emotion; and to a certain extent Professor Sergi, though more nearly in harmony with Lange, agrees with James in this. In the second place, Professor James seems to distinctly include other "motor" sensations and centripetal impulses from musculature other than visceral and vascular, among those which causally contribute to emotion. Thirdly, he urges his theory as one which is completely competent only for the "coarser" emotions, among which he instances "fear, anger, love, grief." For Lange and Sergi the basis of apparition of all feeling and emotion is physiological, visceral, and organic, and has seat for the former authority exclusively, and for the latter eminently, in the vasomotor system.

This view, which some may conceivably tax with "materialism,"† has a merit that materialism does oftentimes possess, namely, relative accessibility to experimental test.‡ Such test it is attempted in certain measure to apply in the observations which I herewith report. They have been obtained from five young dogs. In these

* 'Ztschf. f. Psychologie u. Physiologie d. Sinnesorgane,' Hamburg and Leipzig, 1897, vol. 14, p. 93.

† James, 'Principles of Psychology,' vol. 2, p. 453.

‡ Sollier, 'Revue philosophique,' Paris, March, 1894. Dr. Sollier records experiments made on subjects in the condition of deep hypnosis; their sensation, both cutaneous and deep, was believed to be abolished; the conclusions he draws from the experiments are in support of the theory of James and Lange.

the spinal cord has been severed in the lower cervical region. Such a severance lies headward of the exit and entrance of all that system of nerves usually embraced under the term "*sympathetic system.*" It therefore sunders from the brain all nexus with the thoracic, abdominal and pelvic viscera, except that existent through certain cranial nerves. It also cuts off all the blood vessels from the bulbar vasomotor centre, except for certain scanty communications through the cranial nerves. The skin and motor organs are, as far as the shoulder, likewise cut off from all communication with the brain. Therefore behind that level they are precluded from contributing to nervous processes of emotion, either in their centripetal or their centrifugal phases.

In each of these dogs the observations have been prolonged for several months subsequent to the operation of transection; in none has any impairment whatever of emotional character, so far as demonstrable, been detected. To study emotion in a lower animal is not altogether easy—even in a dog. But if reliance be placed on the signs that are usually taken to signify pleasure, anger, fear, disgust, then these animals showed them as unmistakably after as prior to the transection of the cervical spinal cord. The sight of, or the sound of, the attendant who kept them evoked from them the same joyous activity and animated caressful pose of head and feature as formerly. Towards friends and enemies among their fellow-inmates of the animal house they displayed as markedly as ever their liking or their rage. To give an instance, I saw fear notably displayed by one of the dogs, a young animal, approached and threatened by a powerful old Macaque monkey. The lowering of the head, the dejected half-averted face, and the drooped ears contributed to indicate existence of an emotion as lively as the animal had ever shown us before the spinal operation had been made.

An observation of confirmatory kind I once obtained in the laboratory of my friend Professor Mosso of Turin. In a young dog under deep chloroform narcosis, I had performed a spinal transection close behind the origin of the phrenic nerves. Six weeks later, the trauma having completely healed and the condition of spinal shock having largely subsided, I placed the animal once more under chloroform, but this time not profoundly. I connected the femoral artery with the mercurial kymograph and proceeded to record the arterial pressure, allowing the chloroformisation gradually to pass off. As the depth of the narcosis waned, the breathing became quicker and less regular. The waking of the animal was accompanied by no pain, because the whole body was insentient behind the cervical region, and the kymograph attachment was in the femoral region. I was intending to faradise a branch of one of the nerves of the right hind limb. Inductorium, electrodes, galvanic cells, and whole electric circuit stood

on a table near, but not on that on which the kymograph observation was in process. In order to be sure that all was ready, I closed the electric key and touched the vibrator of the inductorium. The harsh rattling noise of the vibrator lasted a few seconds, and I then stopped it by re-opening the key. Turning thereupon toward the arterial record, I was a little disappointed to see that a marked oscillation had suddenly upset the already somewhat undesirably irregular line that had to serve as starting level for the vasomotor reflexes I was wishful to study. It was clear that one would have to wait for greater quietude to re-establish itself again. I waited; the disturbance of the arterial pressure subsided; the previous fairly equable cardiac beat, despite somewhat disquiet respiration, returned. A few minutes later I again started, by force of habit, trying the inductorium for a couple of seconds preparatory to proceeding to excite and observe the vasomotor reflexes. Again, on turning toward the trace running on the kymograph, I was met by a sudden disturbance that had altered it. This time it occurred to me that the sudden whirring noise of the magnetic interruptor might have caused the reaction. This supposition I proceeded to test, and soon found that each time the noise was repeated the disturbance of the circulation followed. If the reaction had become less, as it frequently did after a number of repetitions, it was only necessary to wait for ten minutes or a quarter of an hour in order to re-obtain it in its original extent.

I then remembered that in examining the limits of the cutaneous anæsthesia in this animal from week to week, I had at several times employed the inductorium; sometimes the electrodes had in making the delimitation been applied to points of skin still sentient, and no doubt had there caused sensations of unpleasant quality. The recurrence of the sound to the awakening animal occasioned now emotional anxiety. But in this animal the vasomotor centre cut off by the spinal section from practically the whole of the rest of the vasomotor mechanism was quite unable to affect the arterial pressure.* Hence that rise of pressure observed by Couty and Charpentier† to occur under emotion of fear was impossible in this case. All the more obvious and uncomplicated for that reason appeared the inhibitory action exerted on the heart. The heart that had been beating at the rate of 180 per minute, suddenly fell for twenty seconds to a rate of 54 per minute. The respiratory rhythm was easily seen to be also altered, but no graphic record of the respiratory movement was being employed. A slight elevation of the mean arterial tension immediately preceding the

* Some description of the *spinal* reflex and other vasomotor reactions obtained from these animals I hope to give in the 'Journal of Physiology'; they are not necessary to the argument here.

† "Effets cardio-vasculaires des excitations des sens," fig. 4. 'Archives de *Physiologie normale et pathologique*,' 1877, p. 560.

vagus action on the heart I incline to attribute to mechanical effect on the circulation, secondary to alteration in respiratory movement. The

FIG. 1.—Record of the arterial pressure in a dog forty-one days after spinal transection at the 7th cervical segment. The arterial pressure is high and good in spite of the transection, the period of vasomotor shock having passed by. For the short period marked by the signal the noise of the vibrator of an inductorium sounded and was heard by the animal. The point of the signal marked nearly 8 mm. further to the right than did the kymograph pen. The inhibition of the heart is shown by the oscillations on the kymograph trace. The kymograph paper moved from right to left, so that the tracing reads from left to right. The line marked "*Zero of B.P.*" signifies the height of the zero of the manometer recording the arterial pressure.

interest of the observation here is that it gives an objective illustration of a disturbance emotional in character occurring in an animal

after the possibility of vasomotor reaction had been set aside, and after the vastly larger portion of all visceral reaction had also been removed.

All the evidence I obtained from all the dogs went absolutely concordantly to show that in spite of exclusion of such a huge field of vascular, visceral, cutaneous, and motor reaction the emotional states of anger, delight at being caressed by the master or at approach of a friend, fear, and disgust were developed with as far as could be seen unlessened strength. The horripilation of the coat along the crest of the back between the shoulders, so usual an accompaniment of anger in the dog, was of course absent in these dogs, the spinal pilomotor* nerve-fibres having had all connection with the brain ruptured. But absence of this reaction could not for a moment mask emotional disturbance so vividly indicated by other features of expression. Regarding emotions of fear and disgust, the former was evoked by threatening with the voice or gesturing with a whip, the latter by the expedient of substituting dog's-flesh for horse- and ox-flesh in the dog's food-pan. Few dogs, even when hungry, can be prevailed on even to touch dog's-flesh as food; almost all turn away from it at once with obvious signs of repugnance and dislike. Fear and disgust in answer to these tests seemed as indubitable in these dogs as in normal. Great care was taken throughout not to establish in the dogs under observation by too frequent repetition or encouragement a habit or trick of response by emotional signs that might thus become so to say pseudo-emotional with the artificiality of an acted performance. No employment of the special tests for eliciting the emotions was made in them until after performance of the spinal section. Even then the tests were never frequently rehearsed; nor were the animals ever encouraged or incited to respond unduly or in a particular manner. It was sought to as far as possible approximate the tests to natural incidents, and to as far as possible collect the observations from natural incidents.

The above was the condition found to obtain in the animals after the cervical spinal transection. I then proceeded in two animals to carry the test further by additional severance of both the vagi nerves in the neck. The vagus may be regarded as the great visceral unit of the cranial series of nerves. Its section subsequent to prethoracic spinal transection relegates to the field of insentience the stomach, the lungs, and the heart, in addition to the other viscera previously rendered apæsthetic.† It also limits still more narrowly the number

* Langley and Sherrington, 'Journal of Physiology,' Cambridge and London, 1891.

† By apæsthetic is meant not only devoid of sensitivity but deprived of all connection with the nervous centres necessary to conscious reaction, a meaning for which the word *apæsthesia* was suggested by Dr. Mott and myself, these 'Proceedings,' vol. 56, 1895.

of efferent and afferent channels by which the vascular system can be possibly affected.

Of the animals chosen for these further observations, one was selected because we soon noted marked emotional characteristics in her behaviour even on her first arrival in the laboratory. She was a mongrel-bred fox-terrier with rather wiry coat, white in colour. She was older than the other dogs; her exact age we did not know. She quickly showed herself affectionate toward the laboratory attendants, one of whom had her in charge; but toward some persons and toward several inmates of the animal house, she frequently exhibited violent displays of anger. Her ebullitions of rage were sudden. Their expression accorded well with a description of the symptoms of rage in the dog furnished by Darwin.* Besides the utterance of the growl, "the ears are pressed closely backwards, and the upper lip is retracted out of the way of the teeth, especially of the canines." The mouth was slightly opened and lifted; the eyelids widely parted; the pupils dilated. The hair along the mid-dorsum, from close behind the head to a point more than half way down the trunk, became rough and bristling. A particularly violent outburst of anger was once suddenly, without warning, exhibited against a visitor who happened to enter with me, and had not before visited the room. Spinal transection in the cervical region was performed on this animal (under deep anæsthesia). Subsequent examination months later at the autopsy proved the section to have been through the 6th cervical segment, where it trenches on the 7th. The severance was complete, as was confirmed by microscopic examination. Rapid recovery from the trauma followed. An interval of depression of the spinal functions behind the site of lesion was succeeded by gradual restoration of reflex activity, surface temperature, &c. Sensation, superficial and deep, was found to be lost behind the limit shown by the skin line indicated in the accompanying figure (fig. 2, p. 400, the lower diagram). The flexors of the elbow were not paralysed, but the extensors were completely so. I have shown† that the sensory nerve supply and motor nerve supply to any muscle have both of them the same segmental position in the spinal cord. Therefore the only muscle still sentient behind the shoulder region must have been the diaphragm.

No alteration whatever was detected in consequence of this lesion in the occurrence of emotion, as judged by anger, by delight, or, when provocation arose, by fear. Her joy at the approach or notice of the attendant, her rage at the intrusion of a cat with which she was unfriendly, appeared as active and thorough as before. But among the signs expressive of rage the bristling of the coat along the back no longer occurred. On the other hand, the eyes were

* "Expression of the Emotions," Darwin, London, 1872, p. 117.

† 'Phil. Trans.,' London, 1897.

well opened, and the pupil distinctly dilated in the paroxysm of anger. Since the brain had been by the transection shut out from discharging impulses *via* the cervical sympathetic, the dilatation of pupil must have occurred by inhibition of the action of the oculomotorius centre. That the cervical sympathetic had been cut off from its normal bulbar and cerebral excitation was shown by the semi-paralysis of the membrana nictitans in this dog, as in all the others, after cervical spinal transection. This partial closure of the eye, due to impaired tonus in the third eyelid, was little if at all diminished during the outburst of rage; but we sometimes thought that during the fit of anger the third eyelid was a little more retracted than in its usual paretic condition.

As in the other dogs after spinal transection, so in this, the spinal transection markedly enfeebled the voice. This we thought traceable entirely to the enfeeblement of the respiratory muscles, the only respiratory muscle left unparalysed after the transection being the diaphragm. The viciousness of the enfeebled and short-winded growl and bark remained as unmistakable and virulent as ever. Apart from this change in the ocular and vocal factors of the facial and respiratory expression of anger, we detected no departure from their previous normal in any direction whatsoever. The heart-beat could be felt to be altered, sometimes becoming quick and sometimes slow, but rarely remaining unchanged during the exhibition of wrath. I thought I could feel "*vagus beats*," but the upset of respiratory movement made the judgment difficult.

One hundred and eighty days after the spinal transection, I divided under deep chloroform anæsthesia the right vagus nerve in the neck, about the level of the cricoid cartilage, and therefore well below the superior laryngeal branch, but above the recurrent laryngeal. The cervical sympathetic trunk in the dog is contained in the same sheath as the vagus, so also is the *depressor* branch of the superior laryngeal, and all three were divided by the same section. This operation produced curiously small obvious result. There ensued little or no difference between the pupils; the right was generally a little the smaller. Absolutely no difference was discoverable between the degree of protrusion of the third eyelids right and left. The palpebral openings on the two sides appeared the same. The pose of the pinna of the ear of each side, both right and left, seemed quite similar. The voice, after the first day succeeding the operation, when it seemed altered in some quality difficult of description, reassumed the character it had had since the spinal transection. The exhibition of emotion, as tested by delight, anger, and fear, indicated emotional states as marked and violent as ever. The trauma was rapidly recovered from as regards the healing of the small wound necessary.

Twenty-eight days later the left vagus nerve was similarly divided under deep anæsthesia, and at the same level as the right. The left

depressor nerve and the left sympathetic trunk were severed at the same time. Subsequent examination at the autopsy proved that both nerves had been completely cut. An additional guarantee was given by the absolute absence of all effect on stimulating the distal end of each of the four trunks before proceeding to the autopsy—that is, twenty-one days after the second vagotomy, when complete nerve degeneration had been allowed time to occur.

In this animal, the superior laryngeal was the lowest branch of the vagus remaining intact and connected with the brain. The recurrent laryngeals proceeded from the vagus trunks, below the level of the sections. The results of the operation were dyspnœa tending to occur in short-lasting attacks, but often passing off entirely; some loss of appetite, which was in the course of seven days recovered from; considerable enfeeblement of, and alteration of, the growl and bark, both these however still remaining, although modified. The attacks of dyspnœa diminished, and in the course of ten days disturbed the animal rarely and but little. We began to think they might be avoided altogether. The animal seemed quickly to learn what postures were least hampering to respiratory movement, and this had as result a marked improvement in breathing and general condition.

In this animal the capacity of the nervous system differed from that obtaining in those subjected solely to the spinal transection, in that to the body regions and organs already cut off from the brain and rendered anæsthetic and put beyond power of contributing to conscious reaction, there were added in this case the stomach and lower half (?) of œsophagus, the lungs and lower half (?) of the trachea, and finally the heart itself. (Compare diagrams, fig. 2.)

Of any diminution or change in the emotional character of the animal we could detect no trace. The following illustrates her condition in that regard.

The approach of the visitor whose advent months previously elicited such violent anger, again provoked an exhibition of wrath as significant as before. The expression was indubitably that of aggressive rage. The animal propped itself against its kennel, and followed each movement of the stranger as though of an opponent, growling viciously, and barking in spite of increasing dyspnœa under the excitement. On other occasions a cat with which she was never friendly, and a monkey new to the laboratory, approaching too near the kennel, excited similar ebullitions. No doubt was left in our minds that sudden attacks of violent anger were still easily excited in this animal. She also gave evidence daily that she experienced the accession of joyous pleasure and delight she had always shown at the approach of the attendant the first thing of a morning, or at feeding time, or when caressed by him, or encouraged by his voice.

I had carefully refrained from testing this animal previously with

regard to disgust at dog's-flesh if it were offered in her food. After her recovery from the last operation, that is to say from the eighth

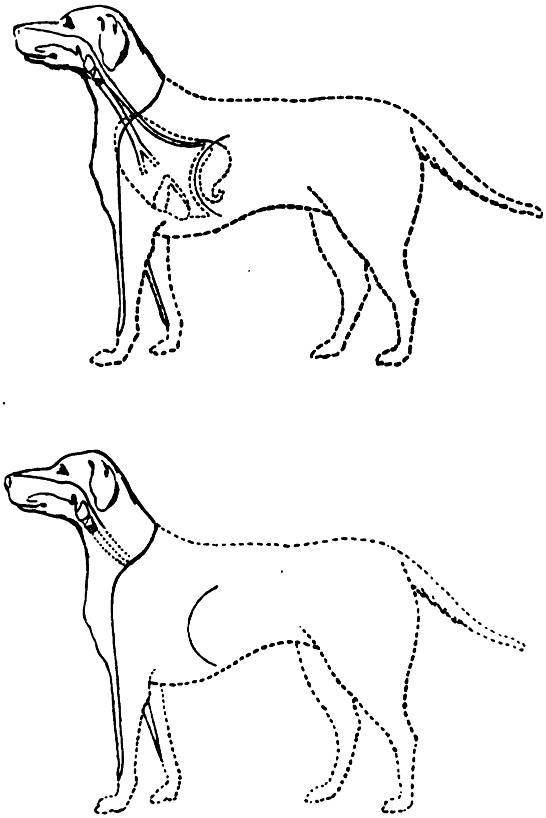


FIG. 2.—Diagram to indicate the extent of the parts still retaining sensitivity after the spinal (upper figure) and combined spinal and vagosympathetic (lower figure) nerve sections described in the text, pp. 393, 396. The extent of skin surface left sentient is delimited by the continuous (not dotted) lines in the figures. The limit of "deep," *i.e.*, muscular, articular, &c., sensitivity also corresponds with this line. But the limit to which the respiratory and alimentary tracts still retained sensation is shown by dotted outlines of the lungs, heart, and stomach in the upper figure, of the larynx and upper part of œsophagus in the lower figure. From anatomical data it is presumed that the trachea and œsophagus had been deprived of all sensitivity somewhere about those levels. The curved line behind the chest indicates the diaphragm as the only muscle behind the shoulder still retaining afferent nerves.

day after it, we proceeded to this observation. Flesh was given her daily in a bowl of milk, and this (after return of her appetite a week *subsequent to* the second vagotomy) she took with relish. The meat

was always already cut into pieces of a size rather larger than the lumps of sugar usual for the breakfast table. It was generally horse-flesh, sometimes ox-flesh. On the tenth day after the final operation the bowl was placed by the attendant, as usual, in the corner of the stall, with milk and meat in it in every way as usual; but the meat was flesh from a large dog killed in the laboratory on the previous day. Our animal eagerly drew itself toward the food; it had seen the other dogs fed and evidently itself was hungry. Its muzzle had almost dipped into the milk before it suddenly seemed to find something amiss there. It hesitated, moved its muzzle about above the milk, made a venture to take a piece of the meat, but before actually seizing it stopped short and withdrew again from it. Finally, after some further examination of the contents of the bowl (it was usual for it to begin the attack of its food by taking out and eating the pieces of meat), without touching them, the creature turned away from the bowl and withdrew itself to the opposite side of the cage. Some minutes later, in result it seemed of encouragement from us to try the food again, it returned to the bowl. The same hesitant display of conflicting desire and dislike was once more gone through. The bowl was then removed by the attendant, emptied, washed, and horse-flesh similarly prepared and placed in a fresh quantity of milk was offered in it to the animal. The animal once more drew itself toward the bowl and this time began to eat the meat, soon emptying the dish. This test was similarly applied afterwards on various occasions; always with the above result, except that twice the animal did, after much hesitancy, lap some of the milk out of the bowl, although dog's-flesh was immersed in it. We have occasionally seen a normal dog do likewise when hungry. To press the flesh upon our animal was of no real avail on any occasion; the coaxing only succeeded in getting her to, as it were, re-examine but not to touch the morsels. The impression made on all of us by the dog's behaviour has been that there existed in the dog's-flesh something which was repulsive to the animal and excited in it disgust unconquerable by ordinary hunger. Some odour attaching to the flesh seemed the mark for its recognition.

Fear seemed to be clearly elicitable in this animal. While I held her in my arms the attendant, approaching from another room the door from which was open, chid the dog in high scolding tones. The creature's head sank, her gaze turned away from her advancing master, and her face seemed to betray dejection and anxiety. The respiration altered and became unquiet, but the pulse was never altered in rate although perhaps slightly in volume.

Twenty days after the last vagotomy the animal suddenly developed a serious attack of dyspnœa; this was recovered from, but in the course of the following day a similar attack occurred. Fearing

lest in our absence such an attack should recur and prove fatal, precluding the possibility of testing the excitability of the vagi and sympathetic and of thus examining the completeness of their functional removal, I therefore on the 21st day after the latter vagotomy and the 229th after the cervical transection killed the animal under deep chloroform narcosis.

A second dog, a quite young puppy, was made the subject of a course of similar experiments. The animal was hardly so suitable, because it at no time, either before commencement of or in the course of the experiment, exhibited to similar degree the signs of anger. Its exhibitions of joy and pleasure, as also, on occasion, of fear, were well marked. The spinal transection in the cervical region, and the double section of the vagi and the cervical sympathetic nerves in the neck, seemed not to dull at all its emotional character as tested by these tests. The spinal transection was through the seventh cervical segment, the section of the vagi above both recurrent laryngeal branches and of the sympathetic trunks at the same level. The animal was preserved under observation 174 days from the time of the first operation, namely, from the spinal transection. The autopsy proved all the sections to have been complete. The results have substantiated those noted in the previously related observations. The only point of difference worth remarking seems that in this last case the reactions were given by a very young animal practically reared in the laboratory, whereas in the just mentioned older bitch there was a past history, with habit and education, of the details of which we in the laboratory knew nothing and have failed to obtain information.

These experimental observations yield no support to the theories of the production of emotion quoted at the opening of this communication. On the contrary, I cannot but think that they go some way toward negativing them.* A vasomotor theory of the production of emotion seems at any rate rendered quite untenable. I am not sure if I understand Professor Sergi aright when I think that he suggests that in absence of vascular and visceral reactions, the very quality of affective tone must be lacking to sensations. Such a suggestion is opposed by the ease with which evidence of unpleasant, *e.g.*, painful quality of sensation could be evoked by appropriate excitation of the still sentient regions of skin in the animals we had before us, the subject of this note.

It need hardly be added that the importance of the concurrence, together with the other reactions in emotion, of marked vascular and

* All who have visited and seen the animals, the subject of this communication, have fully concurred in the opinion of myself and others in the laboratory as to the possession by them of ample and lively emotions. I would especially mention and thank for their attention to the matter, Dr. Abram, Professor Paul, Dr. Warrington, Sir James Russell, and Dr. James Mackenzie.

visceral and cutaneous, seems to me as to others hardly over-estimable for the study of the subject. Wundt,* A. Mosso,† Alf. Lehmann,‡ Head,§ and Wright|| are among those who have in recent years laid stress on that aspect of the phenomenon. I would not be thought to impugn the importance of the study of such organic phenomena in connection with emotional mental states. The only respect in which the here given observations affect the position of affairs is, that they, I think, render it necessary to attribute to these elements of emotion another significance than that imputed by the authorities quoted in my opening paragraph. The picturesque incisiveness of all that comes from Professor James's pen, renders the more persuasive any argument that it pursues. His suggestive chapters led to the above attempt at examination of his theory, an examination the incompleteness of which I wish to unreservedly acknowledge.

The expenses of this investigation have been in part met by a grant given by the Research Committee of the British Medical Association. I would take this opportunity of tendering to them my best thanks for their generous support.

"On the Brightness of the Corona of April 16, 1893. Preliminary Note." By H. H. TURNER, M.A., F.R.S., Savilian Professor. Received March 29,—Read May 10, 1900.

The visual brightness of the corona was measured at the total eclipses of August 29, 1886, and April 16, 1893, by Professor T. E. Thorpe, using a method arranged by Sir W. Abney ('Phil. Trans.,' A, 1889, p. 363, and 1896, p. 433). Soon after the first of these eclipses, Sir W. Abney devised a method of measuring the brightness photographically, by exposing a portion of the plate, which was not exposed to the sky, to a standard light passed through a row of small square screens of varying and known thickness. The result is a series of "standard squares" on the plate, which show the density of deposit due to standard lights of known values; and by comparing the density of the coronal image we find the brightness of the corona in terms of these standard lights.

These squares were first put on the coronal photographs by English observers at the eclipse of December, 1889, and have been systematically used by them since. The 1889 photographs have not yet been measured. Some measures of the 1893 photographs were made by me in Sir W. Abney's laboratory at South Kensington, in July,

* 'Grundriss d. Psychologie,' vol. 1, 3te Auflage, Leipzig, 1893.

† 'La Paura,' Milano, 1885.

‡ 'Das Gefühlsleben,' Leipzig, 1892.

§ "Visceral and referred Pains," Parts I, II, and III, Brain, 1893–7, London.

|| *Ibid.*, "The Physiological Element in Emotion," 1894, London.

1894; but the standard squares had not received a sufficiently long exposure and additional experiments were required. These were carried out by Sir W. Abney during the year; but causes which need not here be dwelt on (chiefly the desire to make further measures which other work has hitherto prevented but which are now being made) have delayed the publication of the results far too long: and, while still reserving the details for a more complete account, I publish one or two general results which may be useful to others in preparing for the forthcoming eclipse.

Three plates were measured, taken by Serg. Kearney, at Fundium. He was provided with a "double-tube," which took photographs of two sizes, the diameters of the moon's image being 0·6 inch and 1·5 inches respectively. The following table shews the details of exposure:—

Table I.

Plate numbers.		Exposures.	
0·6 inch moon.	1½ inch.	Duration.	Time in seconds from commencement of totality.
		seconds.	
1	1A	20	13 to 33
2	2A	120	41 „ 161
3	3A	50	169 „ 219
4	4A	5	227 „ 232
5	5A	2	240 „ 242
6	6A	1	250 „ 251
Totality ends ..	.. ..	.. ..	250

The plates measured were those numbered 3A (large scale), 3 and 5 (small scale), of which the first was measured rather elaborately along four radii extending due N., S., E., W. from the limb respectively, and the reference table or curve of standard lights was constructed for this plate.

The following table shows the actual measures, each number being deduced from the mean of three sector readings by application of the curve of standard lights.

The unit of the table is the effect caused by an amyl acetate lamp shining on the plate from a distance of 9 feet for 1 second.

The numbers represent powers of 2, giving the intensities in terms of this unit. Thus 7·7 means $2^{7\cdot7}$, or 208 times the unit.

The distances from the limb are in terms of a cardboard scale, and it was found by measurement of the moon's diameter that

$$100\,\text{div.} = 15'\cdot6 = \text{solar radius nearly.}$$

Hence 10 div. may be regarded as 0·1 solar radius.

Table II.

Measures of Plate 3A (large scale).

Exposure 50 seconds along four radii (N., S., E., W.).

Distance from moon's limb.		Powers of 2 representing intensity of light.			
		N.	S.	E.	W.
Div.	′				
0	0·0	7·7	8·3	7·8	7·8
10	1·6	..	8·2	..	..
20	3·1	7·7	7·9	..	..
30	4·7	..	7·6	..	7·5
40	6·2	6·9	7·4	7·8	7·0
50	7·8	6·1	6·8	..	6·6
60	9·4	5·5	6·3	..	..
70	10·9	4·8	5·8	..	5·4
80	11·5	4·4	5·1	5·5	4·7
90	13·0	3·8	4·6	..	4·2
100	15·6	3·4	4·0	..	..
110	17·2	3·0	3·5	..	3·1
120	18·7	2·8	3·2	3·3	2·4
130	20·3	2·3	2·8	..	2·8
140	21·8	2·5	2·7	..	..
150	23·4	2·2	2·6	..	2·3
160	25·0	2·2	2·4	2·5	2·2
170	26·5	..	2·3	..	1·8
180	28·1	1·9	2·0	..	..
190	29·6	..	1·8	..	1·7
200	31·2	1·8	1·7	1·9	1·6
210	32·8	..	..	..	1·7
220	34·3	1·5	..	..	..
230	35·9	..	..	..	1·2
240	37·4	1·2	..	1·5	1·4

This table shows—

(1) The accuracy of the method. The intensity of the light is clearly determinable within an error of 0·1 or 0·2, *i.e.*, of $2^{0·1}$ or $2^{0·2}$, which are ratios of 1·07 and 1·15 respectively.

(2) The intensity falls off in nearly the same manner in all four directions. [It may be remarked that 1893 was near a sun-spot maximum, and the corona of the kind approximating to symmetry all round the limb.] Such differences as there are do not arise from the excentric position of the sun behind the moon; for the photograph was taken after the middle of totality, when the moon would have advanced towards the east, and hence the western radius should be brighter than the eastern at the same distance from moon's limb; whereas the contrary is the case. Also there is a marked difference between the N. and S. radii.

(3) The falling off in intensity is very rapid at first. At one radius from the limb it has fallen to one-twentieth, at two radii to one-hundredth.

(4) As regards the absolute intensity, it will be more convenient to refer the brightness to the more familiar unit of the moon's brightness. It is a fair assumption to take the total brightness of the moon as 0·02 candle at one foot; or (since an amyl acetate lamp = 0·8 candle) to $0{\cdot}02 \times 81 \div 0{\cdot}8$ of the units (amyl acetate lamp at 9 feet) adopted in the intensity scale: that is, to 2·025 of such units. Now if the moon were to shine for 50 seconds (the exposure of the photograph) instead of 1, we must multiply this number by 50; and if further it shines on the plate through a 4-inch object glass, so that the light falling on this 4-inch circle is collected into an image of 1·5 inches diameter, the brightness of any point of this patch would represent

$$2{\cdot}025 \times 50 \times (4/1{\cdot}5)^2$$
$$= 720 \text{ units} = 2^{9\cdot5} \text{ units}.$$

Thus if we subtract 9·5 from all the numbers in Table I, we shall get numbers fairly expressing the coronal brightness in terms of that of the moon, in powers of 2 as before.

(5) The results from the other photographs need not be given (in this preliminary note) in detail: they confirm those already given remarkably well; and show that the diminution of light is very gradual indeed after 45 minutes from the limb. They also seem to show that the readings near the edges of the plate in 3A are too low by about 1·0; which is due to the fact that the magnifier is too small to take in the whole object glass at the edges of the field.

(6) We can now give the comparison of visual and photographic observations. The results from plates 3 and 5 are preferred to those of 3A far from the limb, for the reason stated in the last paragraph, and for measures near the limb, results obtained from special measures with a plain glass reflector.

In 'Phil. Trans.,' A, vol. 180 (1889), pp. 380—1, Abney and Thorpe give their visual readings for the 1886 eclipse in terms of a Siemens' unit, and remark that the moon would have given an image equal to 1·2 candles or 1·4 Siemens' units on the same scale. Hence we must divide their figures by 1·4 to get results comparable with the above; and the same must be done for the 1893 eclipse, the numbers for which are given in 'Phil. Trans.,' 1896 (A), p. 433. We thus get the following table of comparative results, replacing now the powers of 2 by ordinary decimals :—

Table III.

d = distance from *limb* in solar radii.	Brightness compared with moon.			Calculated $0{\cdot}05/(d+0{\cdot}18^2)$
	Observed visual.		Observed photographic.	
	1886.	1893.	1893.	
0·0	..	..	(0·40)	(1·54)
0·1	..	..	0·49	0·64
0·2	..	..	0·33	0·35
0·4	..	..	0·18	0·15
0·6	0·047	0·043	0 090	0·083
1·0	0·038	0·034	0·026	0·036
1·4	0·031	0 027	0·017	0·020
1·8	0·024	0·022	0·012	0·013
2·2	0·019	0·017	0·0104	0·0088
2·6	0·015	0·013	0·0092	0·0064

No measures of brightness were made visually within 0·6 of radius of the sun from the limb. It would be interesting to have thi comparison made. For the region where we have a comparison, i appears that the light falls off photographically more rapidly tha visually. This is in accordance with experience, the faint extension having been seen more easily than photographed.

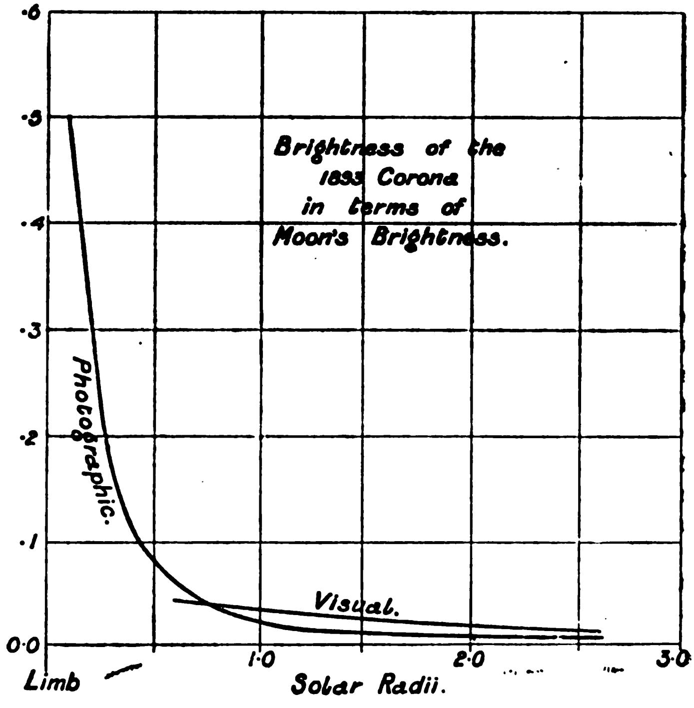
As regards the central portions, we have some indirect information for the *total* brightness of the 1893 corona was found to be equivalen to 0·026 Siemens' unit at one foot or 0·022 candle, *i.e.*, rather mor than the value assumed above for the moon.

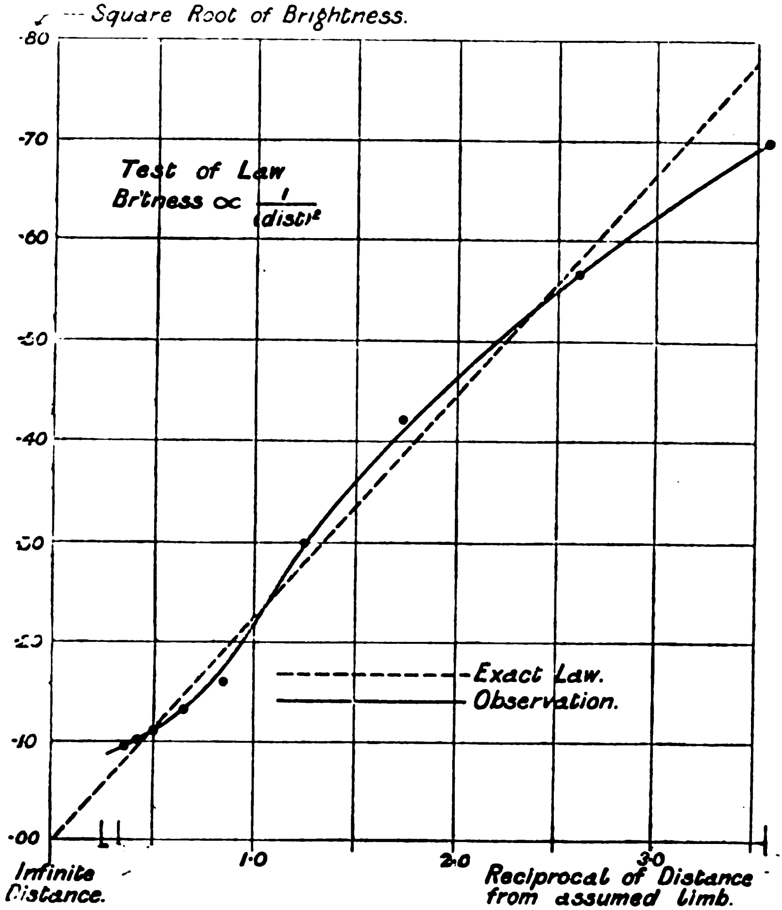
Integrating numerically for the part in the annulus extending fror 0·6 radii to 2·6 radii, we find that this portion is equal to 0·20 moo photographically and 0·25 moon visually. This leaves about 0·7 moon (visual) for the part within this distance (*i.e.*, from the limb t 0·6 radius), while photographically the value got from the curve is onl 0·44 moon. It seems as though the corona were altogether brighte visually than photographically, in the ratio of about 3 to 2; but thi conclusion needs confirmation.

An attempt has been made (in the column "calculated" of th above table) to represent the brightness by a formula. The America photographs of 1878 suggested that the coronal light varied inversel as the square of the distance from the sun's limb. Abney and Thorp find that this law does not hold; but the photographic observation can be made to obey the law approximately. The calculated number are obtained from the formula

$$0{\cdot}05/(d+0{\cdot}18)^2,$$

where *d* is the distance from moon's limb given in first column. The distance is thus measured from a point 0·18 = 2′·7 within the circle on the photograph taken as the moon's limb. This is well within the sun's limb, though near it. It need not cause much surprise that the calculated numbers close to the limb exceed the observed; for the corona close to the limb was obscured during part of the exposure by the advancing moon. Further examination of these points is required. The following diagrams exhibit graphically the figures of Table III.





"Radio-activity of Uranium." By Sir WILLIAM CROOKES, F.R.S. Received May 3,—Read May 10, 1900.

[PLATE 5.]

1. The researches of M. Henri Becquerel have shown that compounds of uranium possess the property now called "radio-activity"; that is, rays emitted by them affect a sensitive photographic plate through bodies usually considered opaque to light; they discharge an electrometer when brought near it; and they are deflected by a magnet. These rays are now called "Becquerel rays," or "uranic rays."

2. On the discovery by M. and Mdme. Curie of polonium and

radium, bodies of enormous radio-active powers, it was suggested that uranium might possibly owe its power to the presence of a small quantity of one of these bodies. But in a paper published in the 'Revue Générale des Sciences' for January, 1899, Mdme. Curie says:—"This does not appear probable, for if such were the case different samples of uranium compounds would have very different radio-activities, but in the course of a number of experiments made with various samples of metallic uranium, as well as with oxides and salts from various sources, I have never found any marked difference between the relative activities of the same compound."

In another paper* the same author says that "the property of emitting rays . . . which act on photographic plates is *a specific property of uranium and thorium*." "The physical condition of the metal seems to be of an altogether secondary importance." "Uranium and thorium alone are practically active."

3. When the discovery of radium was announced, and it was said to have "to all appearance the properties of almost pure barium,"† it occurred to me that radium might be found in detectable quantities in some barium minerals were search made among them from different localities. Accordingly specimens of the following minerals were put on sensitive plates, a sheet of black paper separating them from the sensitive surface. As it was probable that the radio-active substance would be present, if at all, in very minute quantities, the sensitive plate was exposed to their influence for forty-eight hours.

Barytes (Heavy Spar).
„ from Hungary. (Three specimens.)
„ „ Cumberland. (Eight.)
„ „ Westmorland. (One.)
„ „ Cumberland. (A fine crystal.)
„ „ Derbyshire. (Three.)
„ „ Arkendale. (One.)
„ „ Hartz. (One.)
„ „ Scotland. (One.)
„ „ Ireland. (Two.)
„ „ Northumberland. (Two.)
„ „ Arran. (One.)
„ „ Cherbourg. (One.)
„ Several unnamed, but finely crystallised, specimens.
Witherite from Lancashire. (One.)
„ „ Cumberland. (Four.)
„ „ Northumberland. (One.)

* M. and Mdme Curie, 'Comptes Rendus,' vol. 127, p. 175; 'Chem. News,' vol. 78, p. 49, July 29, 1898.

† M. and Mdme. Curie and M. Bémont, 'Comptes Rendus,' vol. 127, p. 1215; 'Chem. News,' vol. 79, p. 1, January 3, 1899.

Not one of these minerals showed the slightest action on the sens tive plate.

4. Having obtained negative results with barium compounds, went through every mineral in my cabinet—a somewhat extensiv collection, numbering many fine specimens. Large photographi plates were covered with black paper, and the minerals were laid o them as close as they could conveniently be placed, accurate note c their names and positions being recorded. They were exposed i total darkness for forty-eight hours. By this means a list of radi active minerals was ultimately obtained. They were then tested fc order of intensity of action. The following is a list of active minera arranged in order, the most active heading the list:—

1. Pitchblende.	9. Broggerite.
2. Uranite.	10. Monazite.
3. Autunite.	11. Xenotime.
4. Orangite.	12. Arrhenite.
5. Thorite.	13. Sipilite.
6. Euxenite.	14. Fergusonite.
7. Samarskite.	15. Chalcolite.
8. Alvite.	16. Hielmite.

It will be observed that these minerals all contain either uranium c thorium.

5. Pitchblende was the most radio-active mineral, but it varie much in different parts. A slice was cut from a piece of pitchblend from Cornwall and the surface was polished. A sensitive phot graphic plate was pressed against it, and after twenty-four hours th plate was developed. The impression showed the structure of th mineral in a remarkable manner, every little piece of pitchblend showing black, those portions in which the radio-active substance wa not so operative showing in half tint, while the felspar, quartz, pyrite &c., having no radio-activity, left the plate transparent (see Plate 5).

Pitchblende from different localities differed greatly in action.

6. A large crystal of orangite from Arendal was ground flat an polished at one end and side, and a piece of sensitive celluloid filn cut half through so as to allow it to bend sharply, was put on th polished surfaces, one half pressing against the end and the othe half against the side. The exposure was continued for seventy-tw hours. On developing, no difference could be seen in the intensity o the impression, whether made by the end or the side of the crysta The impression also was uniform over the surface, the cracks in th surfaces not having impressed themselves.

These experiments were repeated with the interposition of a thi sheet of celluloid between the mineral and the sensitive plate. Th results were practically the same as before.

7. Roughly speaking, the action of pitchblende is in proportion to the percentage of uranium in the mineral. Finely powdered pitchblende of different degrees of richness were experimented with. Cells were made of thick lead pipes half an inch internal diameter and one inch long, closed at the lower ends with card. They were filled with powdered pitchblende, one containing 43 per cent. Ur_3O_8 and the other 12 per cent. Ur_3O_8. A sensitive plate being covered with black paper, the lead cells were laid on it and kept in total darkness for 120 hours. The intensity of the spot under the 43 per cent. ore on development was found to be at least three times that of the one under the 12 per cent. ore.

Two lead cells were taken, one being a quarter of an inch long and the other two inches long. They were filled completely with the 43 per cent. ore, and a sensitive plate exposed to their action for forty-eight hours. On developing it was doubtful whether any difference existed in the intensity of the two spots, proving that the action does not pass through much thickness of active material, a quarter of an inch being equal in effect to two inches.

No difference in the action was noticed when the bottom of the cell was made of thin glass cemented on, instead of card.

Four cells were filled with pitchblende and placed side by side on a sensitive plate. After having acted twenty-four hours the first was removed, the second after forty-eight hours, the third after seventy-two hours, and the last was kept on for ninety-six hours. On developing the plate the spots had intensities varying with the lengths of exposure, and in about the right proportion, on the assumption that double the time of action gives double the intensity of blackening.

8. For convenience of comparison I had a number of glass cells made, three-quarters of an inch wide and deep, so that they could either be sealed up or closed with a cork. A piece of apparatus was made so as to take radiographs of samples with more ease and certainty. A lead plate, 2 mm. thick, $6\frac{1}{2}$ inches long, and $2\frac{1}{8}$ inches wide, has circular holes punched in it, one inch in diameter. Under the thick plate of lead is another thinner plate, made of pure assay foil, and having holes in it, concentric with the others, but barely $\frac{3}{4}$ inch in diameter, so that one of the small cells will not pass through the lower hole, but will pass easily through the upper hole, and thus be kept in place. To prevent contact between the lead plate and the sensitive surface a thin sheet of celluloid is fixed beneath, with holes punched in it concentric with those in the lead plates. In the top left corner of the lead plates is a short steel pin, which can be pressed on the sensitive plate and so register its position in respect to the cells experimented with. The lead and celluloid plates are then bound together and the whole is fitted into a shallow wooden tray with a *light-tight* cover.

A sensitive film is laid face upwards at the bottom of the wooden tray; on this are put the lead screens, and then the experimental cells of radio-active bodies in order, careful note being taken of their relative positions.

9. Wishing to prepare compounds of radium and polonium, the very curious bodies discovered by M. and Mdme. Curie, I arranged with my friend Mr. Tyrer, Stirling Chemical Works, for the systematic working up of half a ton of pitchblende. It was necessary to examine every precipitate and filtrate in each stage of the operation, and for convenience of registration they had to be compared with a standard cell filled with a substance unvarying in action. After many trials, I selected crystallised uranium nitrate as being strongly radio-active and easily prepared pure. This led me to the observation which forms the subject of the present paper.

10. The following compounds of uranium were tested simultaneously, being put into glass cells and arranged on a lead screen with seven holes:—

1. Metallic uranium, from M. Moissan.
2. Uranium nitrate, $UO_2(NO_3)_2.6H_2O$.
3. Uranium acetate.
4. Uranium persulphate.
5. Uranium protosulphate.
6. Uranium oxide (green), UO_22UO_3.
7. Uranium oxide (black), UO_2UO_3.

For twenty-four hours the sensitive plate was exposed to the influence of these bodies. With the exception of metallic uranium, which showed least action, there was not much difference between the effect produced by any of the others.

11. In order to prepare uranium nitrate of great purity for a standard, I took some pounds of the commercial salt and purified it, first by solution in ether (13), and then by repeated crystallisation. After many operations a cell was filled with the crystals, and it was used as a standard. To my surprise, after having acted on a sensitive plate for twenty-four hours, on development not a trace of image could be seen.

12. Thereupon I tried the following experiments to ascertain if the great radio-activity of some uranium compounds and the absence of it in others might be caused by some variation of physical, crystalline, or chemical condition.

Commercial uranium nitrate was taken, and—

(1) A portion was heated with excess of nitric acid to dryness on the water-bath. It was powdered and put in a cell.

(2) A similar portion of the salt was dissolved in alcohol, and the solution evaporated to dryness over the water-bath. The resulting orange-coloured pasty material was ground and put in a cell.

(3) The salt was treated in the same way as No. 1, except that the excess of water and acid were not driven off.

(4) A small quantity was heated for some time on a water-bath till it was thoroughly dry. It was then powdered and put in a cell.

(5) 50 grains were put in a glass cell and heated on a water-bath to about 75° C. till it had dissolved in its water of crystallisation.

(6) 50 grains in a glass cell were heated on a sand-bath to about 230° C. The water of crystallisation having been driven off the salt fused, and on cooling remained a hard, yellow, glassy mass.

(7) A similar quantity of the same salt was heated to a little above 230° C. till it commenced to decompose.

(8) A similar quantity was heated more strongly, till about half the nitrate had decomposed.

(9) The same quantity was heated till decomposition was complete.

(10) As a standard, some of the same lot of commercial uranium nitrate from which these lots were taken was put in a cell.

These ten cells were placed in a lead screen apparatus, and a sensitive plate was exposed to their influence for twenty-four hours. On development there was not much difference between any of the impressions, that under No. 9 being a little the strongest.

Thus it appears that no modification of physical or chemical condition materially affects the radio-active property of a uranium compound when, to begin with, the salt experimented on possesses it; other imilar experiments show that, starting with an inactive uranium salt, nothing that can be done to it will cause it to acquire this property. It is therefore evident that, as I had suspected, the radio-active property ascribed to uranium and its compounds is not an inherent property of the element, but resides in some outside body which can be separated from it.

Having by repeated crystallisation succeeded in preparing a photographically inactive uranium nitrate, I started experiments with several pounds of the commercial salt to ascertain the readiest means of separating from it the active body.

13. Into a stoppered cylinder I put 1 lb. of crystallised nitrate and poured on it a pound of methylated ether, sp. gr. 0·72. The salt easily dissolved on shaking, and after a few hours the whole of the crystals had disappeared, leaving at the bottom of the cylinder 1000 fluid grains of a heavy aqueous solution. I separated the aqueous solution from the ethereal solution, and evaporated it to dryness to remove traces of ether. The ethereal solution was allowed to evaporate spontaneously. Equal quantities of the soluble and the insoluble in ether I put into glass cells and added sufficient dilute nitric acid to dissolve the salt, and then evaporated each lot to dryness on the water-bath. When dry the two cells, and a third containing some of *the original* nitrate, were put on a sensitive plate for twenty-four hours.

On development it was found that the action of the part undissolved by ether was very strong, that of the original nitrate not more than half as strong, while no action whatever could be detected on the part of the plate covered by the salt soluble in ether.

The portion insoluble in ether, after evaporation to dryness with nitric acid, and then crystallisation in water, in no way differed in appearance from ordinary uranium nitrate. The portion soluble in ether, when dried, heated with dilute nitric acid and crystallised, also had the same appearance as the initial salt.

14. The crystallised nitrate from the portion insoluble in ether I again extracted with ether. Most of it dissolved, and a small portion of heavy aqueous liquid settled at the bottom. As before, the nitrate which dissolved in ether had scarcely any radio-active power, while the residue from this second extraction possessed it in a strong degree. The residue after the second extraction was about double the activity of the residue after the first extraction, showing that ether, while dissolving uranium nitrate itself with facility, does not dissolve the body to which it owes its radio-activity.

15. The uranium nitrate from the portion insoluble in ether was submitted to fractional crystallisation in the following manner:—

The solution was evaporated until on cooling about three-fourths would crystallise out. The beaker in which this operation was performed was called No. 1. When crystallisation had finished, the mother-liquor was poured into a beaker called No. 2. A little water was added to No. 1 and it was warmed to dissolve the crystals; No. 2 was evaporated a little, and both were set aside to crystallise. When cold the mother-liquor from 2 was poured into a beaker No. 3, the mother-liquor from 1 was poured into 2, a little water being added to dissolve the crystals in 1, and the contents of the three beakers were warmed and allowed again to crystallise separately. This operation was continued as long as the uranium salt would hold out, or till the tests showed that the operations had gone far enough.

16. Tests were made to see how the operations were proceeding. A portion of the crystals from No. 1 beaker was dried and put into a cell. Some mother-liquor from the last beaker was also evaporated and crystallised, and the crystals were put in a cell. The two were placed side by side on a sensitive plate and the action was allowed to proceed for twenty-four hours. On development there was no visible spot beneath No. 1 nitrate, while that beneath the nitrate from the other end of the fractionation was strong and black.

17. The crystals were removed from their cells, ignited to the green oxide, and replaced. Tested again on a sensitive plate the results were similar to those given by the unignited nitrates. The active substance, therefore, is seen to reside in the mother-liquor.

18. In making photographic tests it is not necessary to take much

of the substance. On an ordinary microscopic slide I put a small drop of liquid from each of the highest five fractionations, Nos. 10, 9, 8, 7, and 6. The drops were allowed to crystallise and the slide was aid on a sensitive plate. In twenty-four hours a good impression of No. 10, the highest fraction, was obtained; a less strong impression of the next, No. 9; a fainter one of 8; a scarcely perceptible one of 7; and no impression at all from No. 6. The slide containing the five crystalline spots was then covered with another glass, and the whole cemented together with Canada balsam and mounted in the manner usual with microscopic slides. When the balsam was dry the slide was put on a sensitive plate. In twenty-four hours a good graduated image was developed.

19. There are in commerce two kinds of uranium nitrate: one, the commercial variety, and another called "purissimum." I am informed that the "purissimum" is prepared from the former by repeated crystallisation. I purchased some of each of these nitrates and tested them on a photographic plate. The commercial variety proved to be at least twice as radio-active as the "purissimum" salt.

20. Experiments were now instituted with a view of obtaining a wholly inactive uranium nitrate. About two pounds of the salt that had been obtained from the solution in ether was repeatedly crystallised, pouring off the mother-liquor each time. This and the next succeeding two lots of crystals (Nos. 1, 2, and 3) were put into cells, and kept on a sensitive plate for seven days. On developing the plate no image could be detected where No. 1 had been; a scarcely perceptible impression could be just detected at No. 2, and a little stronger impression at No. 3.

21. Other methods were attempted for the separation of the active substance from uranium. Uranium nitrate fuses at a moderate temperature, and after some time it becomes darker, and nitrous fumes come off. Finally, the mass becomes semi-fluid, and will not run. The operation is then stopped, and the mass transferred to water; the undecomposed nitrate is dissolved out, leaving an insoluble basic nitrate. The basic nitrate is of an orange-yellow colour, easily dissolved in nitric acid to again form the normal nitrate. By using this method of fractionation the active body gradually accumulates towards the basic end. But the method is neither so complete nor so easily effected as the crystallisation method, and therefore I have not pushed it very far. I have, however, proved that the radio-activity of nitrate of uranium can be concentrated by fractionation to the basic nitrate end, the nitrate at the other end being diminished in radio-activity.

22. A highly active uranium nitrate, prepared by fractionation from the part insoluble in ether, was dissolved in water, and ammonia *in excess* was added. Yellow ammonium uranate was precipitated.

The filtrate was evaporated to dryness, and heated with nitric acid. The yellow precipitate and the residue of the filtrate were put into cells, and laid on a sensitive plate. After twenty-four hours' action the plate was developed, when it was seen that the whole of the radio-activity resided in the ammonium uranate, the other substance showing nothing. This experiment proves that the active body is precipitated by ammonia, and is insoluble in excess.

23. Another portion of active uranium nitrate was dissolved in water, with an excess of ammonium carbonate. The first formed precipitate almost entirely re-dissolved, leaving a small quantity of insoluble light brown flocculent precipitate. This collected on warming, like alumina. It was filtered off, well washed and dried, and put in a glass cell.

The filtrate from the above precipitate was evaporated to drive the ammonium carbonate, when a yellow precipitate came down This was filtered, washed, dried, and put into a cell.

These precipitates were exposed for twenty-four hours in a lead screen apparatus. On developing, it was seen that the residue insoluble in ammonium carbonate instantly flashed out black and dense, while the salt precipitated from the ammonium carbonate solution gave a scarcely discernible image.

24. The action of the precipitate insoluble in ammonium carbonate was so strong that another experiment was tried, exposing the sensitive plate to its action for one hour. On development, the disc of action came out strong and black, although not so black as in the twenty-four hours' experiment. It was now laid for five minutes on a sensitive plate. Here the action was distinct—about as strong as that given by ordinary uranium nitrate in twenty-four hours. These experiments prove that the active body can exist apart from uranium.

If a sheet of thin glass or celluloid is laid on a sensitive plate, and the dried filter-paper with its contents laid on that, and kept down by a weight, an impression is given in as short a space of time as if a glass cell had been used.

25. The radio-active body is not entirely insoluble in ammonium carbonate. A portion of the very active precipitate left after separation from the uranium was dissolved in dilute hydrochloric acid, and an excess of ammonium carbonate added. The precipitate was very brown due to the presence of iron; it was dried and tested. The filtrate was well boiled, and as the ammonium carbonate evaporated a slight precipitate came down. This was collected on a filter and tested by the side of the first precipitate. On developing the plate the images produced by each precipitate were of about equal intensity. The brown precipitate was digested in very dilute hydrochloric acid in the cold. The iron partially dissolved before the rest of the substance, leaving the residue decidedly paler in colour. This pale body

was just as radio-active as before the partial removal of the iron. Therefore the presence of iron does not interfere with the activity of the substance.

26. Having thus definitely proved that the supposed radio-activity of uranium and its salts is not an inherent property of the element, but is due to the presence of a foreign body,* it is necessary patiently to determine the nature of the foreign body. Several radio-active bodies claimed to be new have already been extracted from pitchblende, and experiments have been instituted to see if the newly found body UrX had similar chemical properties to those of the older active substances.

27. Polonium was first tried. A photographic plate had a thin sheet of celluloid laid on it, and over this a sheet of aluminium foil, 0·05 mm. thick. On this double layer were put two cells, one containing basic polonium nitrate, the other active UrX. Action was allowed to proceed for twenty-four hours, and the plate was then developed. A disc of blackening was seen under where the UrX stood, the action having passed through the glass, celluloid, and aluminium. Under the polonium nitrate no trace of action could be detected.

The experiment was repeated, minus the aluminium foil, and the action continued only two and a quarter hours. On development, the UrX was found to have acted well, while the polonium showed no trace of action.

28. This behaviour of polonium being excentric or contrary to published accounts,† I put some polonium nitrate in a very thin gelatine capsule, and laid it for eight hours on a sensitive plate. No trace of an image could be seen on development.

The same polonium nitrate was put in a watch-glass, and the sensitive plate put *over it* face downwards, so that it might be exposed

* For the sake of lucidity the new body must have a name. Until it is more tractable I will call it provisionally UrX—the unknown substance in uranium.

† "The rays emitted by compounds of polonium render barium platinocyanide fluorescent . . . To make the experiment, place on the active substance a very thin sheet of aluminium, and on this a thin layer of barium platinocyanide; in the dark the barium platinocyanide appears feebly luminous over the active substance" (M. and Mdme. Curie and M. Bémont, 'Comptes Rendus,' vol. 127, p. 1215; 'Chem. News,' vol. 79, p. 1). "Polonic rays act on sensitive plates. The substance we call sulphide of polonium gives a good impression after only three minutes, and there is a decided action noticed after even half a minute" (Mdme. Curie, 'Revue Générale des Sci.,' January 30, 1899; 'Chem. News,' vol. 79, p. 77). In the same paper the authoress, after describing the power possessed by a polonium compound to excite phosphorescence, says: "the rays emitted by this latter body have traversed the aluminium and excited the fluorescence of the platinocyanide above it." It is evident from the above extracts that I was justified in thinking that polonium rays were not entirely stopped by *thin aluminium*, glass, or celluloid.

to the direct emanations from the polonium nitrate. On the top of the plate was laid a sheet of lead to press it tight to the edges of the watch-glass.

The exposure was continued for twenty-eight hours. On developing, a strong action was seen, strongest in the middle where opposed to the thickest part of the heap of polonium nitrate, and weaker towards the edge. A well-marked action took place all over the plate exposed to the interior of the watch-glass, but it was sharply cut off at the edges. This confirms the previous results—that the emanations from polonium are of a different character to those from radium or UrX, both of which pass through glass, aluminium, and lead.

29. Another property of polonium sharply distinguishing it from UrX is volatility. The discoverers first obtained it by subliming pitchblende *in vacuo*. Afterwards they used this property to separate it from bismuth, the polonium and the bismuth sulphides depositing at different parts of the hot tube.

A strongly radio-active compound of UrX was ignited in a blowpipe flame with the addition of a drop of sulphuric acid. Its radio-activity, on a sensitive plate, was not diminished by this treatment. This experiment was tried several times at increasingly higher temperatures, and always with the same result.

30. Polonium is precipitated by sulphuretted hydrogen, in an acid solution. An acid or neutral solution of UrX is not precipitated by this reagent. Therefore I am justified in saying my UrX is not polonium.

31. But it is not so easy to settle whether UrX is distinct from radium, although many arguments point to its not being radium. The discoverers of radium give several of its chemical properties, and in most of these UrX and Ra are entirely different. Thus, radium sulphate is said to be insoluble in water and acids, while UrX dissolves easily to a clear solution in dilute sulphuric acid. Radium salts are said not to be precipitated by ammonium sulphide or by ammonia, while UrX is precipitated by both.

32. It was hoped that doubtful points might be settled conclusively by the spectrum, as both radium and polonium give well-defined and characteristic lines, especially in the ultra-violet part of the spectrum where I have chiefly worked. M. Demarçay* has given a list of some of the principal lines in the radium spectrum between the wave-lengths 3649·6 to 4826·3, the one at 3814·7 being very strong, and those at 4683·0, 4340·6, and 3649·6 being next in intensity. He draws special attention to the line at 3814·7 as the line showing first in a compound poor in radium. In none of my UrX compounds have I been able to detect a trace of this line; on the other hand I have

* 'Comptes Rendus,' vol. 124, p. 716; 'Chem. News,' vol. 80, p. 258.

failed to photograph this line in products which I know contain radium. The reason is my radium compound is too weak. M. Demarçay says the line is scarcely visible with a radium compound only sixty times as active as uranium. My substance containing radium was still weaker, judging from its action on a photographic plate.

33. The same reasoning applies to polonium. With polonium I have obtained strong lines in the ultra-violet, but I can detect none of them in the spectrum of my compound of UrX. All that I can see are lines belonging to—

Platinum (from the poles),
Uranium,
Calcium,
Aluminium,

and a few of the strongest air lines, besides a large number of faint lines difficult to identify.

34. Spectrum experiments having failed to show a difference between radium and UrX, it was thought that possibly some information might be gained by submitting them to the radiant matter test, which has proved so fruitful in its application to the yttrium earths. Some of the most active UrX was put in a tube furnished with a pair of terminals, and it was exhausted to a high point, heat being applied during exhaustion. Simultaneously a self-luminous radium compound was sealed in a vacuum tube and exhausted, heat being likewise applied. When fully exhausted a strong induction spark was passed through each tube. The UrX compound phosphoresced of a fine blue colour. In the spectroscope no discontinuity could be seen in the spectrum of the phosphorescent light.

Under the influence of the induction spark, the radium compound phosphoresced of a luminous rose-colour, showing in the spectroscope a concentration of light in the red-orange, and a very faint citron band, due to a trace of yttrium, probably an impurity.

35. A powerful radium compound and one of UrX, each in a glass cell, and a paper tray full of polonium sub-nitrate, were placed side by side, and a strip of white card was put as a reflector at the back. In front a photographic camera was arranged so as to throw full-sized images of the polonium, UrX, and radium compounds on a sensitive plate, and the whole was kept in total darkness for five days. On development the image of the radium with the containing bottle was visible, but not a trace of image from the polonium could be seen. This confirms previous observations that the radiations from polonium will not pass through glass. Those from radium and UrX easily penetrate glass and other media (28).

36. Recently claims have been put forward for the existence of a

third radio-active body in pitchblende. In the 'Comptes Rendus' for October 16, 1899, and April 2, 1900, M. A. Debierne describes a radio-active body, to which he gives the name of "Actinium." At first he said "actinium" showed the principal analytical properties of titanium, but later he describes it as not resembling titanium in all its reactions. M. Debierne gives many reactions of the new substance, and in some instances they are like those of radium. But he qualifies them by the statement that they cannot yet be considered as belonging definitely to the new radio-active substance, because up to the present it has not been obtained sufficiently concentrated. He believes rather that these reactions should be looked upon as the result of retention, analogous to that of iron oxide by barium sulphate. He says that the chemical reactions of the most active substance which he obtained, together with its spectroscopic examination, showed that it chiefly consists of thorium. He cannot, however, be sure that it resembles thorium in all its reactions.

37. Experiments have been commenced to see if it is possible to separate thorium compounds into an active and an inactive body. A strong solution of thorium sulphate was slightly acidulated with sulphuric acid, and gradually raised to the boiling point. A copious precipitate of sulphate came down, and was filtered hot. The precipitate was dissolved in cold water, and the solution re-heated, when a precipitation of the sulphate again occurred. The mother-liquor from one crystallisation was added to the crystals from another in the systematic manner adopted in fractionation, and when the operations had proceeded some time a test was made on the "head" and "tail." A small quantity of solution from each was evaporated to dryness and strongly ignited before the blowpipe. The two lots of earth were put in cells and a sensitive plate exposed to their action for seventy-two hours. On development not the slightest difference could be detected between the impressions produced by either of the fractions.

I next tried partial crystallisation of thorium nitrate, fractionating it in the way already described in the case of uranium (15). Great difficulties were here encountered, owing to the tendency of a strong solution of thorium nitrate to remain supersaturated for several days, when it would suddenly crystallise to a solid mass. After some weeks, however, six fractionations were effected, and tests were made on the first and last of the series.

The sensitive plate was exposed to their action for 120 hours. On development, the fraction at the first end (crystals) gave a very feeble action, while that at the other end (mother-liquors) gave an impression about three times as intense. This points to the possibility of separating from thorium its radio-active substance.

By the kindness of Dr. Knöfler, of Berlin, who makes thorium

nitrate by the ton, I have at my disposal some specially prepared thorium nitrate, which is chemically pure. Thoria prepared from this, tested on a sensitive plate, gave a feeble impression in 120 hours.

38. In the present state of our knowledge of these radio-active bodies it is safest to retain an open, or even a slightly sceptical, mind. We recognise them mainly by the photographic and the electrical tests—reactions which are so sensitive that they give strong results, even when the active body is present in too small a quantity to be detected by its spectrum—one of the most delicate of tests. Knowing the tendency of ordinary chemical bodies to be carried down when a precipitate is formed in their presence, even when no question of sparse solubility is involved, it is not surprising that radium and actinium, to say nothing of UrX, appear to simulate elements which may ultimately prove to be very different from them in chemical characters. For instance, UrX dissolves easily in dilute sulphuric acid, and, I have reason to believe, forms a soluble sulphate; still, when chloride of barium is mixed with it and precipitated as a sulphate, I invariably find strong radio-activity in the precipitated sulphate as well as in the filtrate from the barium sulphate.

To adduce a simile from my previous researches, the first surmises as to the chemical characteristics of the bodies now known to be yttrium and samarium, were widely different from reality. The differences were entirely due to the perturbing cause which is active in the present case—the tendency of the bodies to be carried down and entangled in precipitates, where, according to ordinary chemical laws, they ought not to occur; and to the extreme delicacy of the radiant matter test, which in the case of samarium detects one part in 2½ million parts of calcium, and in the case of yttrium detects one part in the presence of a million parts of extraneous matter.

39. The radiographic test for these active bodies presents another point to be borne in mind. Other tests for the presence of an element either act quickly, or do not act at all, with a comparatively narrow margin of debateable land where the indications of the test may be doubtful. Here, however, the test is cumulative. Like an astronomer photographing stars too faint for his telescope to disclose, he has only to expose the plate for a sufficiently long time and the star reveals itself on development. So, in the case of radio-active minerals or precipitates, if no action is apparent at the end of an hour, one may be shown after twenty-four hours. If a day's exposure will show nothing, try a week's. Considering my most active UrX does not contain sufficient of the real material to show in the spectrograph, yet is powerful enough to give a good impression on a photographic plate in five minutes, what must be its dilution in compounds which require an hour, a day, or a week to give an action?

[redacted]

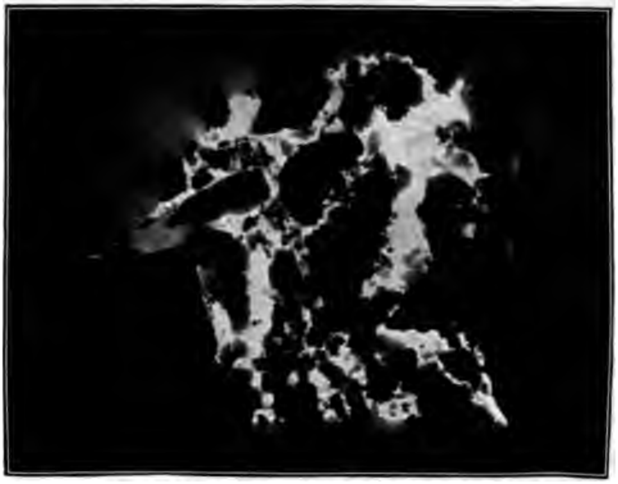


FIG. 2.

FIG. 1.

DESCRIPTION OF PLATE 5.

FIG. 1.—Photograph taken by daylight of a cut and polished surface of pitchblende.

FIG. 2.—Radiograph impressed in the dark by the same surface, showing the portions (white) emitting radiant energy. The luminous parts are pitchblende, the dark parts are felspar, quartz, pyrites, &c. (see para. 5).

May 31, 1900.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

Dr. Robert Bell (elected 1897) was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The President announced that the subscription portrait of Sir John Evans, painted by Mr. A. S. Cope, A.R.A., had been handed over for the acceptance of the Society by the Portrait Committee.

The following Papers were read:—

I. "Palæolithic Man in Africa." By Sir JOHN EVANS, K.C.B., F.R.S.

II. "On the Estimation of the Luminosity of Coloured Surfaces used for Colour Discs." By Sir W. DE W. ABNEY, K.C.B., F.R.S.

III. "The Sensitiveness of Silver and of some other Metals to Light." By Major-General J. WATERHOUSE, I.S.C. Communicated by Sir W. DE W. ABNEY, K.C.B., F.R.S.

IV. "The Crystalline Structure of Metals. Second Paper." By Professor EWING, F.R.S., and W. ROSENHAIN.

V. "Vapour-density of Bromine at High Temperatures. Supplementary Note." By Dr. E. P. PERMAN and G. A. S. ATKINSON. Communicated by Professor RAMSAY, F.R.S.

VI. "Influence of the Temperature of Liquid Hydrogen on Bacteria." By Dr. A. MACFADYEN and S. ROWLAND. Communicated by LORD LISTER, P.R.S.

The Society adjourned over the Whitsuntide Recess to Thursday, June 14.

CROONIAN LECTURE.—"On Immunity with Special Reference to Cell Life." By Professor Dr. PAUL EHRLICH, Director of the Royal Prussian Institute of Experimental Therapeutics, Frankfort-on-the-Maine. Received March 17,—Read March 22, 1900.

[PLATES 6 AND 7.]

(Translation.)

Honoured President, my lords and gentlemen,—It is to me the very greatest honour that I have been summoned here by your most highly esteemed Society, which for more than two centuries has represented and still represents the centre of the scientific life of England, in order that I may deliver the Croonian Lecture. I consider I am not so much personally concerned in the honour that you bestow on me, and that I shall not err if I see in it a recognition of the scientific path which I, in company with many others, have sought to follow, and which in your eyes suffices to place the field in which I work on a footing alongside of exact science. It is an extreme pleasure for me to have the privilege of addressing so many medical colleagues with whom for so many years I have been bound in close ties of friendship, and who have always been the first to welcome and to give recognition to the results of my work.

Since Jenner made his great discovery of the protective action of vaccinia against small-pox, a century has passed away. During these years that terrible scourge of mankind has been almost completely eradicated from the civilised world. The beneficial consequences of Jenner's discovery are so evident to all who have any wish to properly appreciate them, that one wonders why, during so great a portion of the long period of 100 years, they were allowed to stand alone, without any endeavour being made to induce an artificial immunity in the case of other infectious diseases. This is all the more remarkable because Jenner's discovery demonstrated in their entirety those essential principles which, in later times, have been established for other infectious diseases.

In the first place, it was shown that by the use of an attenuated virus, which of itself was non-injurious to the organism, it was possible to ward off the disease caused by the virulent virus. Jenner also established—what is most important from the practical point of view—that by the inoculation of the weakened poison there was produced not only an immediate, but also an enduring, protection. That Jenner's discovery remained so isolated was due essentially to the fact that the theoretical conceptions of the cause and nature of infectious diseases made no advance during the subsequent decades; indeed, it would be an interesting topic for some historian of medicine to trace step by step the gradual advance in the

knowledge of infectious diseases during the past century. Schwann's classical investigations must be regarded as the first link in the long chain. Schwann it was who, in an unusually brilliant manner, first demonstrated that the decomposition of organic bodies in the processes of fermentation and putrefaction was never spontaneous, but constantly arose through the agency of micro-organisms coming from without. This line of investigation reached its zenith in the fundamental work of Pasteur, of which the first and the greatest result—Lister's method of wound treatment—worked a revolution in surgery. Then followed the profound investigations of Koch on Anthrax, and the pure cultivation of the most important pathogenic bacteria.

The work of Pasteur and of Koch afforded the first basis on which the study of artificial immunity could be again undertaken. The possibility of voluntarily producing a number of the most important infectious diseases of men and animals, and of modifying at will pure cultivations of bacteria, either, according to Jenner's precedent, by passage through the animal body, or otherwise in artificial culture media, laid the foundation on which advancement could proceed. Pasteur himself was the first, after Jenner, to produce an artificial immunity by using an attenuated virus; and he was also able to introduce the procedure to some extent into practice with most beneficial results. Still the theoretical explanations of all these facts lagged far behind their practical effects. The very able investigations of Metchnikoff and his theory of phagocytosis were, to many investigators, inconclusive.

With Behring's discovery, that in the blood serum of animals immunised against diphtheria and tetanus, there were contained bodies which were able to specifically protect other animals against the toxines of these diseases, an altogether new factor was introduced into the question. This remarkable discovery seemed at one stroke to open up an entirely new and extremely promising prospect of immunising mankind against the majority of the infectious diseases. It was, therefore, somewhat disappointing when there did not follow, on the successful practical application of diphtheria antitoxic serum, a rapid succession of similar achievements. It may with truth be said, that during recent years there has been somewhat of a standstill in the further following-out of a work at first so enthusiastically received. By purely empirical methods, *e.g.*, by the production and use of sera of very great antitoxic value, the results attained showed no improvement. Better success was only to be hoped for when by an accurate knowledge of the theoretical considerations underlying the question of immunity, explanations of the previous ill-success were forthcoming. Impelled by these considerations I laboured for years trying to shed some light into the darkness that shrouded the subject.

In all exact work with chemical bodies—for only as such can we regard the toxines produced by the living bacteria—the first desider-

atum in the investigation is the exact numerical determination of action and counteraction. The words of the gifted natural philosopher Clerk Maxwell, who said that if he were required to symbolise the learning of our time he would choose a metre measure, a clock, and a kilogramme weight, are equally apposite in reference to progress in the field of inquiry in which we are at present interested. And so at the very beginning of my theoretical work on immunity I made it my first task to introduce measures and figures into investigations regarding the relations existing between toxine and antitoxine. From the outset it was clear that the difficulties to be overcome were extremely great. The toxines, *i.e.*, the poisonous products of bacteria, are unknown in a pure condition. So great is their potency, that we are obliged to assume that the strongest solid (*feste*) poisons which are obtained by precipitating toxic bouillon with ammonium sulphate, represent nothing more than indifferent materials, peptones and the like, to which the specific toxine attaches itself in mere traces beyond the reach of weighing; for up to the present time, by the purely chemical methods of weighing and measuring, it has been impossible to ascertain anything as to their presence or the intensity of their action.

Their presence is only betrayed by the proof of their specific toxicity on the organism. For the exact determination, *e.g.*, of the amount of toxine contained in a culture fluid, the essential condition was that the research animals used should exhibit the requisite uniformity in their susceptibility to the poison. Uniformity is not to be observed in the reaction of the animal body to all toxines. Fortunately in the case of one important body of this nature, viz., the diphtheria toxine, the conditions are such that the guinea-pig affords for investigations the degree of accuracy necessary in purely chemical work. For other toxines this accuracy in measuring the toxicity cannot be attained. It was necessary for me to try to eliminate, as far as possible, the varying factor of the animal body, and bring the investigations more nearly into line with the conditions necessary for experiments of a chemical nature. In the course of these endeavours it was shown that it was possible to obtain in a comparatively simple manner an insight into the theoretical considerations necessary to a proper understanding of immunity, by means of test-tube experiments with suspended animal tissues. The relations were simplest in the case of red blood corpuscles. On them, outside the body, the action of many blood poisons, and of their antitoxines, can be most accurately studied, *e.g.*, the actions of ricin, eel-serum, snake-poison, tetanus toxine, &c. In an experiment of this kind, in which are employed a series of test-tubes containing definite quantities of suspended blood corpuscles, each test-tube represents as it were a research animal, uniform in any one series, and one *that can be* reproduced at will. By means of these test-tube experi-

ments, particularly in the case of ricin, I was able, in the first place, to determine that they yielded an exact quantitative representation of the course of the processes in the living body. The demonstration of this fact formed the basis of a more extended application of experiments of this nature. It was shown that the action of toxine and antitoxine took place quantitatively as in the animal body. Further, these experiments yielded a striking series of facts of importance for the theoretical valuation of the reaction between toxine and antitoxine. It was proved in the case of certain toxines—notably tetanus toxine—that the action of antitoxines is accentuated or diminished under the influence of the same factors which bring about similar modifications in chemical processes—warmth accelerates, cold retards the reaction, and this proceeds more rapidly in concentrated than in dilute solutions. These facts, first ascertained by means of test-tube experiments, have since been confirmed by Behring and Knorr for tetanus within the animal body, and by Martin and Cherry in the case of snake-venom.* The knowledge thus gained led easily to the inference that to render toxine innocuous by means of antitoxine was a purely chemical process, in which biological processes had no share. Yet again insurmountable obstacles seemed to present themselves to this conclusion.

It must be postulated that in chemical processes the bodies sharing in the action react with one another in definite equivalent quantities. This proposition appeared, however, not to hold in the case of the action of antitoxine on diphtheria toxine. When, in the case of diphtheria toxines of different stocks, that quantity of toxine bouillon which is exactly neutralised by a certain definite quantity of diphtheria antitoxine (the official German immunity unit, as laid down for the control examination of sera), was determined, so that every trace of toxic action was abolished, the figures obtained were not in accord. Of one toxine bouillon 0·2 c.c., of another 2·5 c.c., were so neutralised by one immunity unit. Such a relation need not have given rise to surprise, because it was well known that the diphtheria bacillus, according to outside circumstances, yields in the bouillon very different quantities of toxine. It was therefore allowable to infer that the different quantities of toxine bouillon, which were saturated by one immunity unit, were exact expressions of the toxicities of the various bouillons, or, to use other words, indifferently whether the bouillon was strongly or feebly toxic, the same multiple of the minimal lethal dose would be constantly neutralised by one immunity unit,

* *Note during revision.*—The credit of first drawing attention to these points belongs to Professor Fraser, who, as far back as 1896, carried out extraordinarily precise experiments on the conditions of neutralisation in respect both of time and of amount of snake-poison and anti-venin (Lecture, Royal Institution, March 20, 1896).

so that in every case the law of equivalent proportions would hold good.

But when looked into more closely, the relations showed themselves to be by no means so simple. In what manner could one obtain a satisfactory estimation of the strength of a toxine? As the constant factor in such an estimation, it was only possible to proceed from a previously determined standard reaction in the case of a definite species of animal, and so we came to regard as the "toxic unit" that quantity of toxic bouillon which exactly sufficed to kill, in the course of four days, a guinea-pig of 250 grammes weight.

When we employed this standard unit, or "simple lethal dose," to estimate the amount of toxic bouillon neutralised by one "immunity unit," the facts which presented themselves were far more surprising than it was possible to have foreseen at the outset. These results were, that of one toxine, perhaps 20, of a second, perhaps 50, and of yet a third, it might be 130 simple lethal doses were saturated by one immunity unit. Since, however, we had previously assumed that the simple lethal dose alone afforded a standard on which reliance could be placed in determining the combining relations of toxine and antitoxine, it appeared from these results that the neutralisation of toxines by antitoxines did not follow the law of equivalent proportions, and, notwithstanding all earlier work in agreement with such a conception of the action, we were obliged to conclude that between toxine and antitoxine a purely chemical affinity did not exist. The seemingly inexplicable contradiction between the results just stated and previous work was very soon explained. When the neutralisation point of toxine and antitoxine was investigated for one and the same sample of poison, the following results were obtained. Immediately on its preparation, fresh from the incubator, it was found that one immunity unit neutralised α c.c. of toxic bouillon, and this quantity represented β simple lethal doses. When the same toxic bouillon was examined after a considerable interval, the remarkable fact was discovered that exactly α c.c. of the toxic bouillon were again neutralised by one immunity unit; but that these α c.c. now represented only $\beta - x$ simple lethal doses. It therefore followed that the toxic bouillon had retained exactly the same combining affinity, but possessed feebler toxicity. From this it was evident that the toxic action on animals and the combining capacity with antitoxine represented two different functions of the toxine, and that the former of these had become weakened, while the latter had remained constant.

Treated from the chemical standpoint, this circumstance was most simply explained by assuming that the toxine was characterised by the possession of two different combining groups: one, which may be designated *haptophore*, conditions the union with antitoxine, while the *other group*, which may be designated *toxophore*, is the cause of the

toxic action. From the constancy of the combining capacity, and the diminution in the toxicity, it was to be inferred that the toxophore group was very unstable, but the haptophore group more stable, and also that the deterioration of the toxophore group proceeded of necessity quite independently of any relation to the haptophore group.

If we now designated a toxine molecule, of which the toxophore group is destroyed, but its haptophore group retained, as "toxoid," then the above-described process will represent the quantitative progress of the conversion of the toxine molecules into toxoid molecules. Such a toxoid molecule has the same quantitative combining affinity for antitoxine as the original toxine molecule, in spite of the disappearance of toxicity to the animal body. In other words, the affinity of the haptophore group for the antitoxine is absolutely independent of the existence of a toxophore group. Also, in the original toxine molecule, both groups must be to such a degree non-related or independent of one another, that a mutual reaction between them does not take place. This conception of the constitution of diphtheria toxine, after more extensive, very exact, and much varied experimentation, based on its partial neutralisation by antitoxine, has been confirmed in the completest manner possible. At this time it would be superfluous for me to enter into all the details pertaining to these investigations. It need only be remarked that in principle the same relations have been established for tetanolysine by Madsen, for snake-poison by Meyers, and for the milk-curdling ferment by Morgenroth.

The separation of the characteristic atom groups of the toxine molecule into a haptophore and a toxophore group, afforded not merely a satisfactory chemical explanation of the process of neutralisation: the possession of the knowledge of the existence of these groups yielded us, at the same time, the key to the nature of the toxic property of toxines, and to the mystery of the origin of the antitoxines themselves. After it had been established by the already described method, that the toxine molecule was possessed of a definite haptophore group, which accounted for its capacity to enter into combination with other bodies, it was immediately necessary to inquire into the question whether, and if so to what degree, this group entered into the causation of the symptoms of illness. That chemical substances are only able to exercise an action on the tissue elements with which they are able to establish an intimate chemical relationship is a conception of a general nature, which has been entertained since the birth of scientific medicine.

It is astonishing, almost astounding, that this axiom, of which the theoretical importance has been so long recognised, and which has served indeed as the first ground for certain therapeutical procedures, should as a matter of fact have played in the building up and furtherance of scientific pharmacology a *rôle* so insignificant in proportion to its great

importance. In glancing through the modern text-books of pharmacology, with rare exceptions, as, *e.g.*, Stokvis, one finds absolutely no mention of the distribution of drugs in the organism, a matter which is of so much moment for arriving at a true comprehension of the relations existing between pharmacological action, location in the organism, and chemical constitution. As a matter of fact, the methods for obtaining any knowledge of the exact distribution of drugs in the body are as yet very imperfect. Even if we can prove that certain alkaloids are again recognisable as being, *e.g.*, present in the brain, we are but little further advanced in our knowledge of the process, because we cannot determine in which cells and which system of fibres the alkaloid is localised.

I may say, indeed, that as yet the investigation of the laws pertaining to the minute distribution of a chemical substance in the body is only possible when, as in the case of coloured bodies, these are at once recognisable by the eye. But that it is possible at once to draw conclusions of therapeutic importance from the laws governing the distribution was shown in the case of methylene-blue, in which I was able, knowing its distribution in the body, to anticipate for it certain antineuralgic and antimalarial properties which were both established by subsequent investigation. It may be permitted me to call to mind, that in malaria methylene-blue is especially of service in the case of persons who, on account of susceptibility, cannot be treated with quinine, and that in the hands of Koch it has shown itself of eminent value in hæmoglobinuric fever, since as opposed to quinine it exercises no destructive action on the erythrocytes. If we are not able to discover the principles governing the localisation of common chemical bodies, which can be used in suitable quantities in chemical purity, and which chemical and other reactions render perceptible, it was *à priori* very unlikely that efforts directed to locating the toxines, which are potent in the slightest traces, and which are bodies we have no means of rendering perceptible to our senses, would be anything else than absolutely without result.

But that this is not so, has been shown by experiments carried out by Professor Dönitz, in the Steglitz Institute, to which, on account of their great importance, I shall refer somewhat extensively. When a rabbit receives a suitable dose of diphtheria or tetanus toxine injected directly into the circulation, the animal remains for many hours well, and then begins to show symptoms of illness, which gradually increase till they end in death. In order to arrive at an explanation of the incubation period, Dönitz determined the amount of antitoxine which, injected intravenously immediately after the toxine, absolutely neutralised the latter. This neutralising dose is able to render all the toxine circulating in the blood innocuous. When, however, the *neutralising dose* so determined was injected not immediately, but

seven or eight minutes after the injection of the toxine, death occurred from tetanus exactly as if no antitoxine had been given. Part of the toxine, equal at least to the minimal lethal dose, must within this time have disappeared from the blood, in which it would have been neutralised, and passed over to the tissues, especially to the brain. The experiments of Dönitz were afterwards confirmed by an investigation conducted in quite a different manner by Heymans, who showed that a research animal from which the blood had been removed immediately after the injection of the minimal lethal dose of tetanus toxine, and replaced by transfusion of fresh blood, succumbed from typical tetanus. In this case, therefore, in that brief interval of time the minimal lethal dose of toxine had passed through the walls of the vessels and been taken up by the tissues.

Regarding the nature of the processes here concerned, a satisfactory explanation was also afforded by the experiments of Dönitz. It admitted of demonstration that the toxine held in the tissues could still be withdrawn from them, if *not* the simple neutralising dose were injected but larger quantities of the same.

The quantity necessary was greater in proportion as the interval elapsing after the injection of the toxine was longer. However, after a definite period was exceeded, all possible doses of antitoxine, even the very greatest, were impotent, notwithstanding that the animal at the time of the injection of the antitoxine had not developed any symptoms. Since a very great number of other chemical substances, narcotics, alkaloids, and other neurotropic bodies, were not in a position to withdraw the toxine once deposited in the central nervous system, and as the property to do so was solely the characteristic of the specific antitoxine, one was obliged to come to the conclusion that the union between the toxine and the tissues, which could only be overcome by means of a specific chemically-related antagonising agent, must itself depend on a chemical combination. One was therefore forced to accept the idea that the central nervous system, that is to say certain ganglion cells in it, possessed atom groups resembling those of the antitoxine, in having a maximum affinity for tetanus poison. The predilection of the nervous system for tetanus toxine, the rapid union of the toxine with the nervous tissue, the gradual onset of the symptoms and their long duration could only be explained by the existence of such toxophil groups. The statement of Dönitz that the tetanophile atom groups are in the guinea-pig essentially confined to the central nervous system, whereas in the case of other species, especially rabbits, these are also present in other organs, is one of prominent importance.

The beautiful experiments of Roux on intracerebral injection of toxine have yielded absolute confirmation of the statement of Dönitz. Roux found in guinea-pigs that the same dose of tetanus toxine was

lethal, whether given by intracerebral or by subcutaneous injection; for rabbits, however, the lethal dose was twenty times greater subcutaneously than it was in intracerebral injection. This can only be explained in the terms of Dönitz's observation, viz., that in the case of direct injection of the toxine into the brain, the toxophile atom groups there present at once seize on all the toxine, while when the toxine is administered through the blood stream, the toxophile groups present in other organs also take up the toxine in equivalent quantities. In the case of rabbits the absorption of the toxine in this way is very considerable: indeed of twenty parts only *one* part finds its way into union with the nervous system.

We now come to the important question of the significance of the toxophile groups in organs. That these are in function specially designed to seize on toxines cannot be for one moment entertained. It would not be reasonable to suppose that there were present in the organism many hundreds of atomic groups destined to unite with toxines, when the latter appeared, but in function really playing no part in the processes of normal life, and only arbitrarily brought into relation with them by the will of the investigator. It would indeed be highly superfluous, for example, for all our native animals to possess in their tissues atomic groups deliberately adapted to unite with abrin, ricin, and crotin, substances coming from the far distant tropics.

One may therefore rightly assume that these toxophile protoplasmic groups in reality serve normal functions in the animal organism, and that they only incidentally and by pure chance possess the capacity to anchor themselves to this or that toxine.

The first thought suggested by this assumption was that the atom groups referred to must be concerned in tissue change; and it may be well here to sketch roughly the laws of cell metabolism. Here we must in the first place draw a clear line of distinction between those substances which are able to enter into the composition of the protoplasm, and so are really assimilated, and those which have no such capacity. To the first class belong a portion of the food-stuffs *par excellence*; to the second almost all our pharmacological agents, alkaloids, antipyretics, antiseptics, &c.

How is it possible to determine whether any given substance will be assimilated in the body or not? There can be no doubt that assimilation is in a special sense a synthetic process—that is to say, the molecule of the food-stuff concerned enters into combination with the protoplasm by a process of condensation involving loss of a portion of its water. To take the example of sugar, in the union with protoplasm, not sugar itself as such but a portion of it comes into play, the sugar losing in the union some part of its characteristic combining reactions. The sugar behaves here as it does, *e.g.*, in the glucosides, from which it can only be obtained through the agency of

actual chemical cleavage. The glucoside itself yields no trace of sugar when extracted in indifferent solvents. In a quite analogous manner the sugar entering into the constitution of albuminous bodies (glycoproteids) cannot be obtained by any method of extraction; at least, not until chemical decomposition has previously taken place. It is therefore generally easy, by means of extraction experiments, to decide whether any given combination in which cells take part is or is not a synthetic one. If alkaloids, aromatic amines, antipyretics, or aniline dyes be introduced into the animal body it is a very easy matter, by means of water, alcohol, or acetone, according to the nature of the body, to remove all these substances quickly and easily from the tissues. This is most simply and convincingly demonstrated in the case of the aniline dyes. The nervous system stained with methylene blue, or the granules of cells stained with neutral red, at once yield up the dye in the presence of alcohol. We are therefore obliged to conclude that none of the foreign bodies just mentioned enter synthetically into the cell complex; but are merely contained in the cells in their free state. The combinations into which they enter with the cells, and notably with the not really living parts of them (Kupffer's paraplastic portions), are very unstable, and correspond usually only to the conditions obtaining in solid solutions, while in other cases only a feeble salt-like formation takes place. I myself in 1887 placed on a sure footing the fact that the nervous system and the fatty tissues allow of alkaloids and aniline dyes being mechanically shaken out of them, as in the poison-detection process of Stas and Otto.

Hence with regard to the pharmacologically active bodies in general, it was not allowable to assume that they possessed definite atom groups, which entered into combination with corresponding groups of the protoplasm. This corresponds, as I may remark beforehand, with the incapacity of all these substances to produce antitoxines in the animal body. We must therefore conclude, that only certain substances, food-stuffs *par excellence*, are endowed with properties admitting of their being, in the previously defined sense, chemically bound by the cells of the organism. We may regard the cell quite apart from its familiar morphological aspects, and contemplate its constitution from the purely *chemical* standpoint. We are obliged to adopt the view, that the protoplasm is equipped with certain atomic groups, whose function especially consists in fixing to themselves certain food-stuffs, of importance to the cell-life. Adopting the nomenclature of organic chemistry, these groups may be designated side-chains. We may assume that the protoplasm consists of a special executive centre (Leistungs-centrum) in connection with which are nutritive side-chains, which possess a certain degree of independence, and which may differ from one another according to the requirements of the

different cells. And as these side-chains have the office of attaching to themselves certain food-stuffs, we must also assume an atom-grouping in these food-stuffs themselves, every group uniting with a corresponding combining group of a side-chain. The relationship of the corresponding groups, *i.e.*, those of the food-stuff, and those of the cell, must be specific. They must be adapted to one another, as, *e.g.*, male and female screw (Pasteur), or as lock and key (E. Fischer). From this point of view, we must contemplate the relation of the toxine to the cell.

We have already shown that the toxines possess for the antitoxines an attaching haptophore group, which accords entirely in its nature with the conditions we have ascribed to the relation existing between the food-stuffs and the cell side-chains. And the relation between toxine and cell ceases to be shrouded in mystery if we adopt the view that the haptophore groups of the toxines are molecular groups, fitted to unite not only with the antitoxines but also with the side-chains of the cells, and that it is by their agency that the toxine becomes anchored to the cell.

We do not, however, require to suppose that the side-chains, which fit with the haptophore groups of the toxines, *i.e.*, the side-chains which are toxophile, represent something having no function in the normal cell economy. On the contrary, there is sufficient evidence that the toxophile side-chains are the same as those which have to do with the taking up of the food-stuffs by the protoplasm. The toxines are, in opposition to other poisons, of highly complex structure, standing in their origin and chemical constitution in very close relationship to the proteids and their nearest derivatives. It is, therefore, not surprising if they possess a haptophore group corresponding to that of a food-stuff. Alongside of the binding haptophore group, which conditions their union to the protoplasm, the toxines are possessed of a second group, which, in regard to the cell, is not only useless but actually injurious. And we remember that in the case of the diphtheria toxine there was reason to believe that there existed alongside of the haptophore group another and absolutely independent toxophore group.

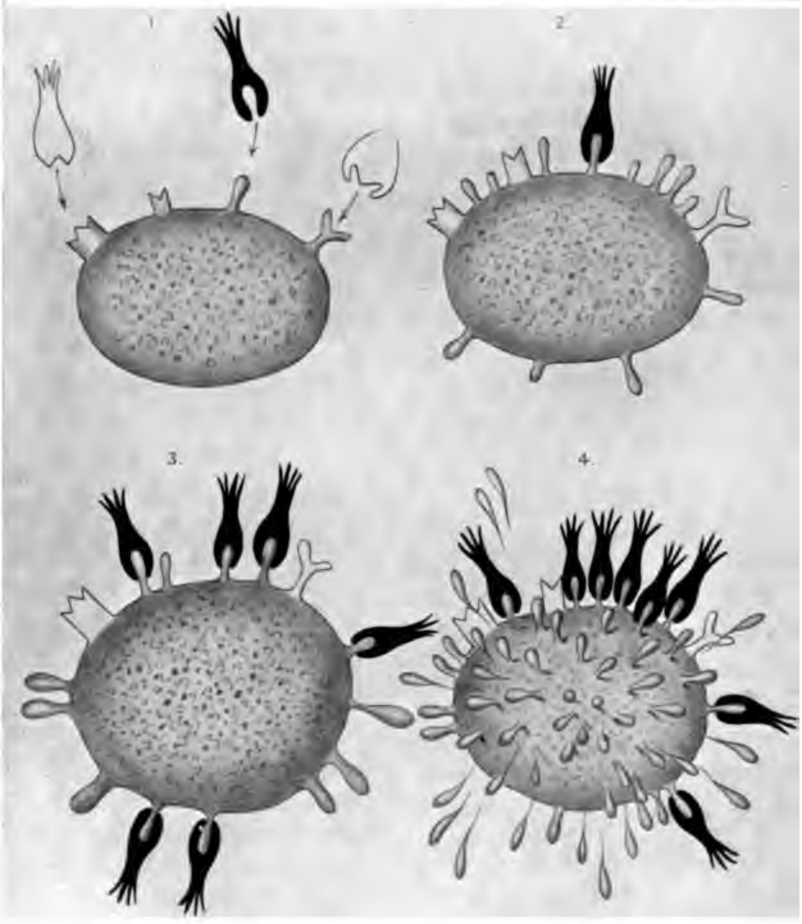
Now for certain cellular elements of the body it can be proved in the test-tube that between these tissues and certain toxines an "anchoring" process takes place exactly similar to that between toxine and antitoxine. Wassermann first demonstrated this in the case of the brain substance. In a mixture of tetanus toxine and broken-down fresh guinea-pig brain the latter so bound or "anchored" the toxine that not only was the surrounding fluid toxine-free, but the brain substance laden with the tetanus toxine had also lost its own toxic action, and so the mixture when injected into an animal was borne without *any harm.*

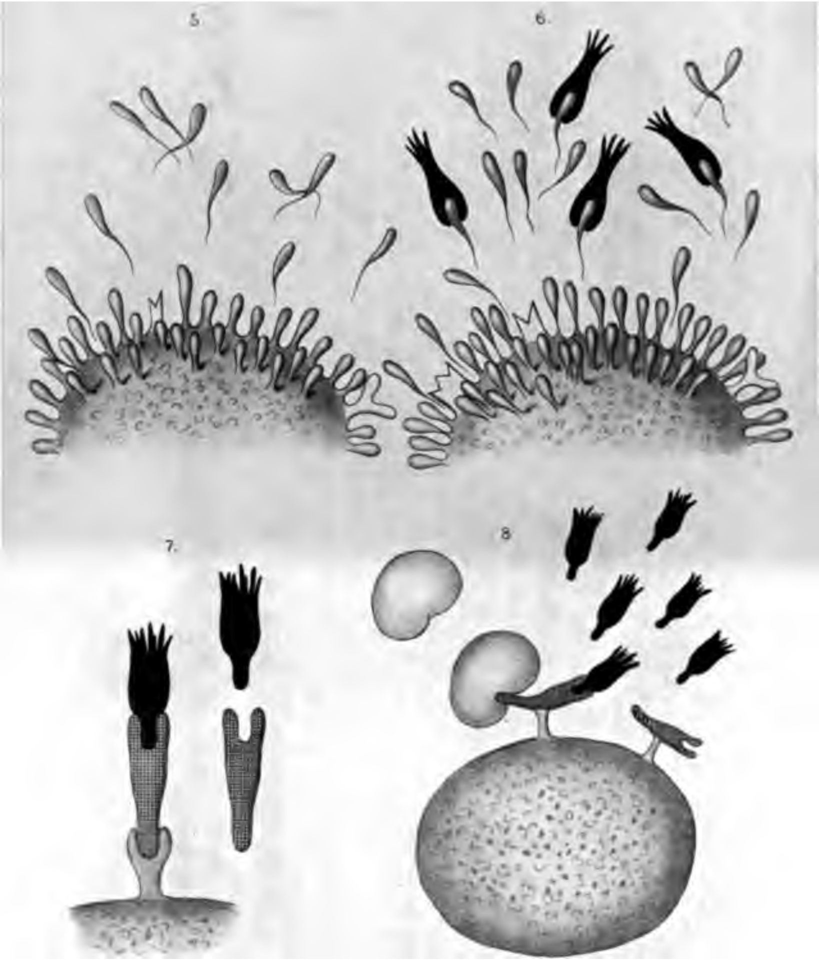
The deduction is, that in this case, a chemical union between the brain substance and the tetanus toxine had taken place, and this was of so firm a nature that on introduction into the body the union was not broken up and therefore the toxine remained innocuous. The brain of the normal animal had, in keeping with my theory, acted exactly like a real antitoxine. There are present in the brain, *i.e.*, in the ganglion cells, tetanophile protoplasmic groups, which unite themselves with the toxine. The presence of such groups is the necessary preliminary and cause of the poisonous action of the tetanus toxine in the living animal. That the process here was not one of simple absorption is proved by the fact that, if the group concerned was destroyed by heat, the brain substance became as incapable of removing the toxine as an emulsion of any other organ of the guinea-pig.

As has been said, the possession of a toxophile group by the cell is the necessary preliminary and cause of the poisonous action of the toxine. This can be most sharply demonstrated in the case of certain blood poisons, viz., the hæmolysines, which exercise a solvent action only on such red blood corpuscles as are able to unite chemically with them. The union with the red corpuscles can be proved, and one has here the great advantage of dealing with living and intact red blood cells instead of broken-down cellular material. Under these conditions it is easy to determine the quantitative relations of the union. If we now regard the action of the toxines with which we are concerned in accordance with the views we have just been discussing, we are obliged to conclude that these are only in a position to act prejudicially on the organism if they are able, by means of their haptophore groups, to anchor themselves to the side-chains of the cells of organs essential to life. If the cells of these organs lack side-chains fitted to unite with them, the toxophore group cannot become fixed to the cell, which therefore suffers no injury, *i.e.*, the organism is naturally immune. One of the most important forms of natural immunity is based upon the circumstance, that in certain animals the organs essential to life are lacking in those haptophore groups which seize upon definite toxines. If, for example, the ptomaine occurring in sausages, which for man, monkeys, and rabbits is toxic in excessively minute doses, is for the dog harmless in quite large quantities, this is because, the binding haptophore groups being wanting, the ptomaine cannot, in the dog, enter into direct relation with organs essential to life. We see, then, that the haptophore groups act especially in bringing definite areas of the cell within the sphere of influence of the toxophore group. In the behaviour of the haptophore and toxophore groups there exists a difference essentially great, as we have already pointed out when referring to the work of Dönitz and Heymans. The haptophore group exercises its activity *immediately* after injection into the organism, while in all toxines—with the perhaps

solitary exception of snake-venom—the toxophore group comes into activity after the lapse of a longer or shorter incubation period, which may, *e.g.*, in the case of diphtheria toxine, extend to several weeks. It is in the highest degree interesting that it is possible, by voluntarily influencing certain of the outside conditions, to exclude absolutely the action of the toxophore group. Courmont has shown that frogs, when kept at a temperature lower than 20° C., manifest no sign of tetanus, even after very large doses of tetanus toxine, but they succumb to fatal tetanus if they are placed in surroundings of a higher temperature. Dr. Morgenroth, working in my Institute, has thrown light on this behaviour by proving that in frogs maintained in cold surroundings the tetanus toxine is fixed in their central nervous system, and that the absence of action at lower temperatures can only be explained by the toxophore group of tetanus toxine having its action restricted within a certain temperature minimum, while independent of this the haptophore group exercises its action on the nervous system at all temperatures.

The theory above developed allows of an easy and natural explanation of the origin of antitoxines. In keeping with what has already been said, the first stage in the toxic action must be regarded as being the union of the toxine by means of its haptophore group to certain "side-chains" of the cell protoplasm. This union is, as animal experiments with a great number of toxines show, a firm and enduring one. The side-chain involved, so long as the union lasts, cannot exercise its normal physiological nutritive function—the taking up of definite food-stuffs. It is as it were shut out from participating, in the physiological sense, in the life of the cell. We are therefore now concerned with a defect which, according to the principles so ably worked out by Professor Carl Weigert, is repaired by regeneration. These principles, in fact, constitute the leading conception in my theory. If, after union has taken place, new quantities of toxine are administered at suitable intervals and in suitable quantities, the side-chains, which have been reproduced by the regenerative process, are taken up anew into union with the toxine, and so again the process of regeneration gives rise to the formation of fresh side-chains. In the course of the progress of typical systematic immunisation, as this is practised in the case of diphtheria and tetanus toxine especially, the cells become, so to say, educated or trained to reproduce the necessary side-chains in ever-increasing quantity. As Weigert has confirmed by many examples, this, however, does not take place as a simple replacement of the defect; the compensation proceeds far beyond the necessary limit; indeed, over-compensation is the rule. Thus the lasting and ever-increasing regeneration must finally reach a stage at which such an excess of side-chains is produced that, to use a trivial expression, *the side-chains* are present in too great a quantity for the cell to carry,


1.
2.
3.
4.


5.
6.
7.
8

and are, after the manner of a secretion, handed over as needless ballast to the blood.

Regarded in accordance with this conception, the *antitoxines represent nothing more than side-chains reproduced in excess during regeneration, and therefore pushed off from the protoplasm, and so coming to exist in a free state.* With this explanation the phenomena of antitoxine formation lose all their strange, one might say miraculous, characters. I have deemed it advisable to represent by means of some purely arbitrary diagrams (Plates 6 and 7) the views I have expressed regarding the relations of the cell considered in the manner I have been describing. Needless to say, these diagrams must be regarded quite apart from all morphological considerations, and as being merely a pictorial method of presenting my views on cellular metabolism, and the method of toxine action and antitoxine formation during the process of immunisation.

In the first place our theory affords an explanation of the specific nature of the antitoxines, that tetanus antitoxine is only caused to be produced by tetanus toxine, and diphtheria antitoxine through diphtheria toxine. This very specific nature of the affinity between toxine and cell is the necessary preliminary and cause of the toxicity itself. Further, our theory makes it easy to understand the long-lasting character of the immunity produced by one or several administrations of toxine, and also the fact that the organism reacts to relatively small quantities of toxine by the production of very much greater quantities of antitoxine. By the act of immunisation, certain cells of the organism become converted into cells "secreting" antitoxine at the same rate as this is excreted. New quantities of antitoxine are constantly produced, and so throughout a long period the antitoxine content of the serum remains nearly constant. The secretory nature of the formation of antitoxines has been very strikingly illustrated by the beautiful experiments of Salmonson and Madsen, who have shown that pilocarpine, which augments the secretion of most glands, also occasions in immunised animals a rapid increase in the antitoxine content of the serum.

The production of antitoxines must, in keeping with our theory, be regarded as a function of the haptophore group of the toxine, and it is therefore easy to understand why, out of the great number of alkaloids, none are in a position to cause the production of antitoxines. Conversely, indeed, I recognise in this incapacity of the alkaloids, in opposition to the toxines, to produce antitoxines, a further and salient proof of the truth of the deduction I have previously based on chemical grounds, that the alkaloids possess no haptophore group which anchors them to the cells of organs. To formulate a general statement, the capacity of a body to cause the production of antitoxine stands in inseparable connection with the presence of a haptophore atomic

group. In the formation of antitoxine the toxophore group of the toxine molecule is, on the contrary, of absolutely no moment. But the toxoid modifications of the toxines, in which the haptophore group of the toxine is retained, while its toxophore group has ceased to be active, possess the property of producing antitoxines.

Indeed, in some cases of extremely susceptible animals, immunity can only be attained by means of the toxoids, and not by the too strongly acting toxines. The toxoids are certainly able to cause the production of antitoxines. To quote an example, it is hardly possible in an animal, which, like the guinea-pig, has all the tetanophile groups confined to the cells of the central nervous system, to produce immunity by means of the unaltered tetanus toxine, whereas this is attained with extraordinary rapidity and ease by means of its toxoids.

The symptoms of illness due to the action of the toxophore group, therefore, play no part in the production of antitoxine. On the contrary, we may consider that the severe symptoms, which indicate injury to the cell-life, disturb the regenerative functions, and thus hinder or entirely frustrate the course of the immunisation process. I have from the first adopted this view, and it was simply a misunderstanding when Knorr, who has been all too soon taken from the field of his labours, affirmed that, according to my theory, sickness of the cell constituted the necessary condition precedent to the new formation and pushing-off of side-chains.

If I am not altogether deceived, the toxoids, where it is a question of producing an active immunisation (and this will always be the case when the immunisation concerns human beings), are destined to play an important *rôle* in practical medicine.

In their theoretical relations the toxoids are also of far-reaching interest, in that they provide a transition to that immunisation which can be called forth by substances which would *à priori* be considered entirely devoid of toxic character, and which are sometimes, like the autochthonous ferments (*i.e.*, those normally present in blood), products of normal cell-life, and in some cases food-stuffs proper. Thus Dr. Morgenroth, working in my laboratory, has proved that the rennet ferment, if introduced in great quantities into the organism, behaves exactly like a real toxine, in that it causes the production of a typical anti-rennet, which up to a certain limit accumulates in proportionally greater quantity, the greater the injected doses. Here, however, we have to do with processes which are altogether within the region of the normal, as is most clearly shown in certain animals, *e.g.*, the horse, in the blood serum of which there is normally present a quantity of anti-rennet, equal to that attained in the goat only after a systematic immunisation carried on for months. The rennet ferment present naturally in the body of the horse is the cause of this great formation of anti-rennet. According to Bordet's experiments, if

injections of milk be given to animals, their serum acquires thereby the capacity to cause flocculent curdling. This action is seemingly rigidly specific because (according to Morgenroth's experiments) the body produced by the injection of goat's milk, coagulated goat's milk, but not human or cow's milk.

The behaviour is also similar when different kinds of albumin, *e.g.*, the sera of different animals or the white of egg, are injected. There appear constantly in the serum of the animal so treated new substances—specific coagulines—which act only in a specific manner, *i.e.*, precipitate only the form of albumin injected. Thus there are produced, by the injection of common food-stuffs, typical "Antikörper," which unite with the substances used to occasion their production, and form with them insoluble combinations.

My investigations have shown me that in the blood of animals which have not been subjected to any treatment we must accept the presence of a number of normal bodies analogous to the "Antikörper," having their origin in the most widely diverse organs, and representing nothing more than nutritive side-chains, which in the course of the normal nutritive processes have been developed in excess and pushed off into the blood.

From all these considerations I think myself warranted in concluding that the formation of antitoxines lacks all the characters of that purposeful, intelligently directed, and remarkable process which it at first seemed to be, and that it is to be regarded merely as a process analogous to those constituting an essential portion of the normal metabolism of the organism. We must admit that the majority of the food-stuffs and of the intermediate products of tissue-change must be able to cause the production and throwing-off of nutritive side-chains. It may be that the new formation only takes place to a limited extent, and that the replacement of any side-chains which have been shut out from their physiological function is all that is accomplished; but the formation may occur in greater proportions, may become excessive, and therefore lead to the presence of "Antikörper" in the blood.

In this way is easily explained the fact of the occurrence in the normal blood serum of antitoxines and of bodies inimical to bacteria, without the animals having ever been brought into relation with the corresponding toxines or bacteria. Here I need only refer to the fact that diphtheria antitoxine is not uncommonly present in normal horses and in men who have never suffered from diphtheria. Particularly weighty in this connection are the observations that have been made on horses, because, on the one hand, these animals never suffer from diphtheria, and, on the other hand, Cobbett has brought forward experimental proof that this normally occurring antitoxine corresponds absolutely as to its properties with the antitoxine produced by artificially immunising. The conclusion, therefore, is that in the body

of the normal horse certain substances may be present which possess side-chain affinities similar to those of the diphtheria toxine, and which, therefore, are quite as capable as the latter are of taking possession of the cell side-chains, and occasioning the regeneration and pushing off of these from the cells; in other words, of causing the presence of an actual diphtheria antitoxine in a normal animal.

Such occurrences direct attention to the possibility of producing immunity in some cases by the administration of definite food-stuffs. Perhaps we have in some such peculiarity of feeding and tissue-change the explanation of the fact so difficult to understand, viz., that individuals of the same race and species react in such diverse manners to the same infection. Certainly we are very far removed from the solution of this important question, which, as yet, has scarcely assumed a tangible form. Still it is our duty to strive with tenacity to overcome the difficulties which surround this point, bearing in mind the words of your illustrious countryman, Francis Bacon: "Sunt certe ignavi regionum exploratores, qui, ubi nil nisi cœlum et pontus videtur, terras ultra esse prorsus negant."

I have now laid before you the fundamental facts which up to the present constitute our knowledge in the field pertaining to immunity, and which can be most easily and successfully explained through the agency of "the side-chain theory." I wish in a few words to dispel some erroneous ideas which have been advanced in opposition to this theory.

Roux has shown that very small quantities of tetanus toxine, if injected directly into the brain, cause the death of the animal. Roux assumes that such an occurrence is not compatible with my theory. Roux is of opinion that according to my theory the brain must be quite immune against tetanus toxine, as the toxophile side-chains of the brain-cells must be identical with the antitoxine, and therefore must exercise an immediate protective action. Experiment showing quite the reverse, the theory is overthrown.

Roux came to this incorrect conception through an erroneous conception of antitoxine. The toxophile side-chains of the brain cells draw directly to themselves the toxine molecules, and, according to my theory, are thus a necessary preliminary condition of the illness. The toxophile groups are therefore really inducers of the action of the poison, and not its preventives.

Those toxophile groups which, like the antitoxines present in the serum, are able to lay hold of toxine immediately on its entry into the blood, and so to divert it from organs essential to life, can alone be regarded as being possessed of any antitoxic action in the true sense of the word. I may be allowed to call to mind Weigert's excellent *simile of iron* and the lightning conductor. Iron attracts electricity,

and is therefore used as a lightning conductor. Great masses of iron present in buildings give rise to, or increase, the risk of their being struck by lightning, and the metal only becomes protective against lightning when it is so employed that the electricity is conducted away outside the building. It would never occur to anyone to speak of great masses of iron machinery present in buildings as if they were lightning conductors. It is equally unreasonable to speak of the antitoxic property of the brain cortex, in which the toxophile groups are present in great quantity, but also retain their relations with the nerve-cells. When this really considerable misunderstanding is eliminated from Roux's results these become entirely confirmatory of my views, and it is difficult to understand how, subsequent to Weigert having placed the matter in so clear a light, the beautiful experiments of Roux can be utilised by another eminent authority as a means of combating my theory.

Much more complex than in the cases hitherto discussed are the conditions when, instead of the relatively simple metabolic products of microbes, the living micro-organisms themselves come to be considered, as in immunisation against cholera, typhoid, anthrax, swine fever, and many other infectious diseases. There then come into existence alongside of the antitoxines, produced as a result of the action of the toxines, manifold other reaction products. This is because the bacterium is a highly complicated living cell, of which the solution in the organism yields a great number of bodies of different nature, in consequence of which a multitude of "Antikörper" are called into existence. Thus we see, as a result of the injection of bacterial cultures, that there arise alongside of the specific bacteriolysines, which dissolve the bacteria, other products, as, for example, "coagulines" (Kraus, Bordet), *i.e.*, substances which are able to cause the precipitation of certain albuminous bodies contained in the culture fluid injected; also the so-much discussed "agglutinines" (Durham, Gruber, Pfeiffer), the antiferments (von Dungern), and no doubt many other bodies which we have not yet recognised.

It is by no means unlikely that each of these reaction products finds its origin in special cells of the body; on the other hand, it is quite likely that the formation of any single one of these bodies is not of itself sufficient to confer immunity. Thus in case of the introduction of bacteria into the body we have to do with a many-sided production of different forms of "Antikörper," each of which is directed only against one definite quality or metabolic product of the bacterial cell. Accordingly, in recent times, the practice of using for the production of immunisation definite toxic bodies isolated from the bacterial cells has been more and more given up, and for this purpose it is now regarded as important to employ the bacterial cells as intact as possible. The beautiful results obtained for plague by

Haffkine, and quite recently by Wright in your own country for typhoid fever, have been arrived at in this way.

The most interesting and important substances arising during such an immunising process are without doubt the bacteriolysines, in the investigation of which Pfeiffer has done such yeoman's service. How really wonderful it is that after the introduction of the cholera-vibrio into the animal body a substance is formed endowed with the power of dissolving the cholera vibrio, and that vibrio only!

This seemingly purposeful and novel phenomenon seems at first sight to have nothing to do with those forces which are normally at the disposal of the organism. It was of the greatest importance to explain the origin of these substances from the standpoint of cellular physiology. The solution offered very considerable difficulties, and was first attained when instead of bacteriolysines, hæmolysines came to be employed in experiments. Hæmolysines are peculiar toxic bodies, which destroy red blood corpuscles by dissolving them. Hæmolysines may occur in a normal blood when they exercise a solvent action on the red blood corpuscles of other species, or they may be artificially produced, in which case, after an animal has undergone a process of immunisation against the blood corpuscles of another species, there appear in the serum hæmolysines which destroy the kind of blood corpuscles employed in the production of the immunity. In their essential characters they are absolutely comparable with the bacteriolysines: but they possess over them the great advantage that they admit of being employed in test-tube experiments, and thus afford opportunity for exact quantitative work altogether independent of the variability of the animal body.

Belfanti and Carbone first discovered the remarkable fact that horses which have been treated with the blood corpuscles of rabbits contain in their serum constituents which are poisonous for the rabbit, and for the rabbit only. While the serum of the normal horse, to the quantity of 60 c.c., could be intravenously injected without harm to the rabbit, a very few c.c. of serum from horses previously so treated with rabbit's blood, proved fatal.

Bordet showed shortly thereafter, that in the case quoted there was present in the serum a specific hæmolysine which dissolved the blood corpuscles of the rabbit. He also proved that these hæmolysines—as had already been shown by Buchner and Daremberg in the case of similarly acting bodies which are present in normal blood—lost their solvent property on being maintained during half an hour at a temperature of 55° C. Bordet added, further, the new fact, that the blood-solvent property of these sera which had been deprived of solvent power by heat, the solvent action could be restored if certain normal sera were added to them.

By this important observation an exact analogy was established with

the facts of bacteriolysis as elicited by the work of Pfeiffer, Metchnikoff, and Bordet. In the work on the Pfeiffer phenomenon of bacteriolysis, it had already been ascertained that the solution of bacteria by specific bacteriolysines was brought about by the combined action of two different bodies: one which was specific, arose during the immunisation and was stable; and another, a very unstable body, which was present in normal serum.

In collaboration with Dr. Morgenroth, I have sought in regard to this question, for which hæmolysis offered prospects favourable to experimentation, to make clear the mechanism concerned in the action of these two components—the stable, which may be designated "immune body," and the unstable, which may be designated "complement"—which, acting together, effect the solution of the red blood corpuscles. For this purpose, in the first place, solutions containing either only the "immune body" or only the "complement" were brought in contact with suitable blood corpuscles, and after separation of the fluid and the corpuscles by centrifugalising, we investigated whether these substances had been taken up by the red blood corpuscles or remained behind in the fluid. The proof of its location in the one position or in the other was readily forthcoming, since to restore to the hæmolysine its former activity, it was only necessary to add to the "immune body" a fresh supply of "complement," or to the "complement" a fresh supply of "immune body," in order that the presence of the hæmolysine in its integrity might be shown by the occurrence of solution of the blood-cells.

The experiments proved that, after centrifugalising, the "immune body" is quantitatively bound to the red blood corpuscles, and that the "complement," on the contrary, remains entirely behind in the fluid. The presence of the two components in contact with blood corpuscles only occasions the solution of these at higher temperatures, and not at 0° C. And an active hæmolytic serum (with "immune body" and "complement" both present) having been placed in contact with red blood corpuscles and maintained for a while at 0° C., it was found after centrifugalising that, under these circumstances also, the "immune body" had united with the red blood corpuscles, but that the "complement" remained in the serum. This experiment showed that both components must, at a temperature of 0° C., have existed alongside of one another in a free condition.

But when analogous experiments were undertaken at a higher temperature it was found that both components were retained in the sediment.

These facts can only be explained by making certain assumptions regarding the constitution of the two components, *i.e.*, of the "immune body" and the "complement." In the first place, *two* haptophore groups must be ascribed to the "immune body," one having a great

affinity for a corresponding haptophore group of the red blood corpuscles, and with which at lower temperatures it quickly unites, and another haptophore group of a lesser chemical affinity, which at a higher temperature becomes united with the "complement" present in the serum. Therefore, at the higher temperature, the red blood corpuscles will draw to themselves those molecules of the "immune body" which in the fluid have previously become united with the "complement." In this case the "immune body" represents in a measure the connecting chain which binds the complement to the red blood corpuscles, and so brings them under its deleterious influence. Since under the influence of the "complement"—at least, in the case of the bacteria—appearances are to be observed (for example, in the Pfeiffer phenomenon) which must be regarded as analogous to digestion, we shall not seriously err if we ascribe to this "complement" a ferment-like character.

It is obvious that when the normal serum of one animal possesses hæmolytic action on the blood of another, the component of the hæmolysine which here unites with the red blood corpuscle and forms the connecting link between it and the "complement" which is essential to the occurrence of solution, cannot, in the absence of any preceding process of immunisation, be designated "immune body." In its characteristics and action, however, it only differs from this in occurring naturally, and may well be designated "intermediate body" (Zwischenkörper). It may here be stated that the constitution of a hæmolysine is graphically represented in fig. 7, Plate 7.

Very important for the conclusion that only with the assistance of the "intermediate body" or of the "immune body" can the "complement," which leads to the solution, become united with the blood corpuscle, is the following experiment. The serum of the dog has very considerable solvent action upon guinea-pig's blood, but loses this property if warmed. If dog's serum, thus rendered inactive by warming, is brought into contact with suspended corpuscles of guinea-pig's blood, these are not dissolved; but, if to such a mixture there be also added guinea-pig serum, *i.e.*, the serum *normal* to these red blood corpuscles, the erythrocytes are at once dissolved. Here the only explanation is that the "intermediate body," which possesses a specific affinity for guinea-pig erythrocytes, and is present in the inactive dog's serum, is able to seize on one of the many "complements" present in guinea-pig's serum, with the result that the "complement" which cannot normally attach itself to the corpuscles, comes now to exercise its destructive influence.

We see at the same time from this experiment that the hæmolysines occurring naturally, obey the same laws as those produced through the process of immunising. In fact, for them also, in a great number of instances, precisely similar behaviour has been demonstrated.

The character of the specific union made it possible to find solutions for a number of important questions. In the first place, regarding the multiplicity of the hæmolysines, which occur normally in serum, it is well known that numerous sera are able to dissolve blood corpuscles of different species. For example, serum of the dog dissolves blood corpuscles of the rabbit, guinea-pig, rat, goat, sheep, &c. The complex nature of these hæmolysines has been already indicated.

Another question arises whether in a serum that is capable of such manifold action there is present one single hæmolysine that destroys different red blood-cells, or whether a whole series of hæmolysines come into action, of which one is adapted to guinea-pig blood, another to rabbit blood, &c. The solution of this question may be approached in another way. The serum may be rendered inactive by heat, and then placed in contact with red blood corpuscles of a given kind. Then, supposing, for example, that rabbit blood has been employed, it is found that if the fluid is freed from the erythrocytes by centrifugalisation and the "complement" afterwards added, it is no longer in a position to dissolve rabbit blood, but has not suffered any impairment of its action on other kinds.

By this method of *elective absorption* it is proved that the normally occurring hæmolysines which chain the blood corpuscles of the rabbit to themselves, are specifically adapted to this purpose. If with suitable adjustment of conditions similar experiments be conducted with other kinds of blood, results are obtained which force us to the conviction that in such a serum acting on various kinds of blood there are present absolutely different "intermediate bodies" (analogues of the "immune bodies"), of which each one is specific for one kind of blood, *i.e.*, one is adapted for rabbit's blood, a second for calf's blood, &c. Dr. Morgenroth and I have in some cases, indeed, succeeded in proving that the "complements" which are adapted to fit themselves to these "intermediate bodies," and occur in normal sera, differ among themselves. If we reflect that in normal blood, in addition to these different hæmolysines, there are besides a long series of analogous bodies, agglutinines of very different kinds, bacteriolysines, enzymes, anti-enzymes, we are brought more and more to the conviction that the blood serum is the *carrier of substances innumerable as yet little known or conceived of.*

Having obtained a precise conception of the method of action of the lysines of the serum—of the hæmolysines, and thereby also of the bacteriolysines—it becomes possible for us to attempt to solve the mystery of the origin of these bodies. I have in the beginning of this lecture fully developed the "side-chain theory," according to which the antitoxines are merely certain of the protoplasm "side-chains," which have been produced in excess and pushed off into the blood.

The toxines, as secretion products of cells, are in all likelihood still

relatively uncomplicated bodies; at least, by comparison with the primary and complex albumins of which the living cell is composed. If a cell of the organism has, with the assistance of an appropriate "side-chain," fixed to itself a giant molecule, as the proteid molecule really is, then, with the fixation of this molecule, there is provided one of the conditions essential for the cell nourishment. Such giant molecules cannot at first be utilised by the cells, and are only made available when, by means of a ferment-like process, they are split into smaller fragments. This will be very effectually attained if, figuratively speaking, the "tentacle" or grappling arm of the protoplasm possesses a second haptophore group adapted to take to itself ferment-like material out of the blood fluid. Through such complex organisation, by which the "tentacle" acts also as the bearer of a ferment-functioning group, this group is brought into close relation with the prey destined to be digested and assimilated.

For such appropriate arrangements, in which the "tentacular" apparatus also exercises a digestive function—if it be permissible to pass from the abstract to the concrete—we find analogies in the different forms of insectivorous plants. Thus it has been known since the famous researches of Darwin that the tentacles of Drosera secrete a proteid-digesting fluid.

If we now recognise that the different lysines only arise through absorption of highly complex cell material—such as red blood corpuscles or bacteria—then the explanation, in accordance with what I have said, is that there are present in the organism "side-chains" of a special nature, so constituted that they are endowed not only with an atomic group by virtue of the affinities of which they are enabled to pick up material, but also with a second atomic group, which, being ferment-loving in its nature, brings about the digestion of the material taken up. Should the pushing-off of these "side-chains" be forced, as it were, by immunisation, then the "side-chains" thus set free must possess both groups, and will therefore in their characteristics entirely correspond to what we have placed beyond doubt as regards the "immune-body" of the hæmolysine.

In this manner is simply and naturally explained the astonishingly specialised arrangement that, through the introduction of a definite bacterium into the body, something is produced which is endowed with the power of destroying by solution the bacterium which was administered and no other. This contrivance of the organism is to be regarded as nothing more than a repetition of a process of normal cell-life, and the outcome of primitive wisdom on the part of the protoplasm.

In conclusion, I wish hastily to touch on only a few points. First, *to* direct attention to the fact that the immunising sera produced by the *administration* of bacteria are sometimes limited in their operation to

certain animal species, and are much more inconstant in their action than are the antitoxines. Sobernheim, in the laboratory of C. Fraenkel, found that the anthrax serum obtained by immunising German marmots (Hamster) protected this species, even in small doses; but was absolutely without action for rabbits. Kitt had a precisely similar experience with symptomatic anthrax. This circumstance is easy to understand, if the complex nature of the lysines be borne in mind. The lysine, be it bacteriolysine or hæmolysine (*i.e.*, "immune body" + "complement"), possesses altogether three haptophore groups, of which two belong to the "immune-body" and one to the "complement." Each one of these haptophore groups can be bound by an appropriate "anti-group." Three anti-groups are thus conceivable, any one of which, by uniting with one of the haptophore groups of the lysine, can frustrate the action of the lysine. To my mind, of these three possible "Antikörper," that one which can lay hold of the haptophore group of the "complement," and so prevent this from uniting with the "immune body," is the most important. Dr. Morgenroth and I have experimentally succeeded in producing such bodies by processes of immunisation, and in proving that they unite with the "complement" (anticomplement).

Dr. Neisser at the Steglitz Institute sought to find an explanation of Sobernheim's experiments. He was able to determine that anthrax serum failed in mice, even if great quantities of fresh sheep's serum (*i.e.*, containing excess of "complement") were at the same time introduced. The failure in this case appears to be due, on the one hand, to the destruction, in the body of the mouse, of the "complement" present in the sheep's serum, and, on the other hand, to the fact that the "immune body" yielded by the sheep does not find in mouse serum an appropriate new "complement."

From this it appears, that in the therapeutic application of antibacterial sera to man, therapeutical success is only to be attained if we use either a bacteriolysine with a "complement" which is stable in man ("anthropostabile complement"), or at least a bacteriolysine, the "immune body" of which finds in human serum an appropriate "complement." The latter condition will be the more readily fulfilled the nearer the species employed in the immunisation process is to man. Perhaps the non-success which as yet has attended the employment of typhoid and cholera serum will be converted into the contrary if the serum be derived from apes and not taken from species so distantly removed from man as the horse, goat, or dog. However this may be, the question of the provision of the appropriate "complement" will come more and more into the foreground, for it really represents the centre round which the practical advancement of bacterial immunity must turn.

A second and at present much-discussed question is the immunising

of the organism against elements standing biologically much higher in the scale than erythrocytes and much less foreign to the body than those exceedingly lowly organisms, the bacteria. I refer here to the production of "Antikörper" against cells of the higher animal organisation, *e.g.*, ciliated epithelium (v. Dungern), spermatozoa (Landsteiner, Metchnikoff, Moxter), kidney cells, and leucocytes. These "Antikörper" are also of a complex nature. They obey the already described law of elective absorption, and their origin is in keeping with the "side-chain" theory. It is to be hoped that such immunisations as these, which are of great theoretical interest, may also come to be available for therapeutic application. The idea has already been mooted by v. Dungern, of attacking epithelial new formations, particularly carcinoma, by means of specific "antiepithelial sera," and Metchnikoff has expressed the somewhat bold hope of being able to delay old age by means of a serum directed against phagocytes (macrophages). But even if in the immediate future no great practical success is attained, we must remember that we are only at the very beginning of a rational investigation of properties of cells which hitherto have been far too lightly regarded.

The sifting of the material obtained by observation is rendered more difficult by the occurrence under normal conditions of a great number of quite unlooked for bodies furnished with haptophore groups and arising from diverse organs, and which we may designate collectively as *haptines*. It is to be expected that the study of these haptines will not only throw light on the more minute details of cellular metabolism, but also prove fruitful in the fields of pathology and therapeutics. By the fact that we can cause the individual haptines of the cells to pass out into the blood serum by a process of specific immunisation, it becomes possible in the test-tube to analyse more accurately the mode of operation of their binding groups than is possible in the case of the complicated conditions which present themselves in the animal body. The importance, for the study of immunity, of considering the circumstances from a purely *cellular* standpoint is evident from all that I have said.

I trust, my lords and gentlemen, that from what I have said you may have obtained the impression, to allude again to my quotation from Bacon, that we no longer find ourselves lost on a boundless sea, but that we have already caught a distinct glimpse of the land which we hope, nay, which we expect, will yield rich treasures for biology and therapeutics.

I desire to express my indebtedness to Dr. E. F. Bashford, McCosh Scholar of the University of Edinburgh, now working with me in my Institute, for his kindness in undertaking the translation of my lecture *into English*, a task to which he has devoted much time and trouble.

June 14, 1900.

Annual Meeting for the Election of Fellows.

Dr. G. J. STONEY, Vice-President, in the Chair.

The Statutes relating to the Election of Fellows having been read, Sir J. Crichton Browne and Professor M. J. M. Hill were, with the consent of the Society, nominated Scrutators, to assist the Secretaries in the examination of the balloting lists.

The votes of the Fellows present were collected, and the following Candidates were declared duly elected into the Society :—

Burch, George James, M.A.
David, Professor T. W. Edgeworth, B.A.
Farmer, Professor John Bretland, M.A.
Hill, Leonard, M.B.
Horne, John, F.G.S.
Lister, Joseph Jackson, M.A.
MacGregor, Professor James Gordon, D.Sc.
Manson, Patrick, M.D.
Muir, Thomas, M.A.
Rambaut, Professor Arthur A., M.A.
Sell, William James, M.A.
Spencer, Professor W. Baldwin, B.A.
Walker, Professor James, D.Sc.
Watts, Philip.
Wilson, Charles T. R., M.A.

Thanks were given to the Scrutators.

June 14, 1900.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Some New Observations on the Static Diffusion of Gases and Liquids, and their Significance in certain Natural Processes occurring in Plants."* By H. T. BROWN, F.R.S., and F. ESCOMBE.

* The title of this paper has since been amended to "Static Diffusion of Gases and Liquids in Relation to the Assimilation of Carbon and Translocation in Plants."

II. "The Electrical Effects of Light upon Green Leaves. (Preliminary Communication.)" By A. D. WALLER, M.D., F.R.S.

III. "The Nature and Origin of the Poison of Egyptian Lotus (*Lotus arabicus*)." By W. R. DUNSTAN, F.R.S., and T. A. HENRY.

IV. "The Exact Histological Localisation of the Visual Area of the Human Cerebral Cortex." By Dr. J. S. BOLTON. Communicated by Dr. MOTT, F.R.S.

V. "Data for the Problem of Evolution in Man. V.—On the Correlation between Duration of Life and the Number of Offspring." By Miss M. BEETON, G. U. YULE, and K. PEARSON, F.R.S.

VI. "The Diffusion of Ions produced in Air by the Action of a Radioactive Substance, Ultra-violet Light, and Point Discharges." By J. S. TOWNSEND. Communicated by Professor J. J. THOMSON, F.R.S.

VII. "On an Artificial Retina, and on a Theory of Vision." By Professor J. CHUNDER BOSE, F.R.S. Communicated by LORD RAYLEIGH, F.R.S.

"On the Periodicity in the Electric *Touch* of Chemical Elements. Preliminary Notice." By JAGADIS CHUNDER BOSE, M.A., D.Sc., Professor of Physical Science, Presidency College, Calcutta. Communicated by LORD RAYLEIGH, F.R.S. Received December 6, 1899,—Read January 25, 1900.

In my previous communication* an account was given of the contact sensitiveness of elementary substances to electric radiation. It was shown that though many substances exhibit a diminution of contact resistance, there are others, of which potassium may be taken as an example, which show an increase of resistance—an increase which, in some cases, lasts during the impact of electric waves, the sensitive element quickly recovering on the cessation of radiation. There are thus produced two opposite effects, which depend on the nature of sensitive substance.

As the normal action of radiation is to produce opposite effects on the two classes of substances, it would be advisable, in order to avoid confusion, to use a simple term to indicate these actions, and to distinguish them from one another, by calling the one positive and the other negative. The sensitiveness is found confined to the outer surfaces in contact, and not extended to the substratum; I have there-

* "On a Self-recovering Coherer, and the Study of Cohering Action of different Metals," 'Roy. Soc. Proc.,' vol. 65.

fore used the term "Electric *Touch*," in the restricted sense of sensitiveness to electric radiation, the touch being regarded *positive* when there is a diminution of resistance by the action of radiation, and *negative* when radiation produces an increase of resistance.

In continuation of my previous investigation, I have completed a systematic inquiry into the action of nearly all the elements, including the metalloids and non-metals; also of alloys, amalgams, and compounds. Very great difficulty was at first encountered in working with the non-metals, which are extremely bad conductors. The difficulty has, however, been overcome by the successful working of a new method of radiation balance. As a result of these investigations, it is found that the increase of resistance exhibited by elements is far from being a rare phenomenon, nearly half the number of elements exhibiting this effect.

In the exhibition of the phenomenon of contact sensitiveness, various causes give rise to actions which appear at first to be very anomalous. These causes have been isolated and their effects separately studied. Results have thus been obtained which are uniformly consistent.

Experiments have specially been carried out in the following subjects:—

(1) On the passage of electricity through imperfect contacts.

(2) The effects of various physical causes on the contact sensitiveness.

(3) On the difference between mass action and molecular action.

(4) On the changes produced in the sensitive substance by the action of radiation.

(5) On the cause of "fatigue."

(6) On electric reversal.

The study of the above supports the following hypotheses:—

(*a*) That the contact sensitiveness depends on the chemical substance.

(*b*) That the sensitiveness of an element is a periodic function of its atomic weight.

(*c*) That the effect of radiation is to produce a molecular change or allotropic modification of the substance acted on, so that a positive substance becomes less positive, and a negative substance less negative. The change may in certain cases produce actual reversal.

(*d*) That the so-called "fatigue" is due to the presence of "radiation products."

A detailed account of these investigations will be communicated at an early date.

"On Electric Touch and the Molecular Changes produced in Matter by Electric Waves."

By JAGADIS CHUNDER BOSE, M.A., D.Sc., Professor of Physical Science, Presidency College, Calcutta. Communicated by Lord RAYLEIGH, F.R.S. Received January 29,—Read February 8, 1900.

[* In the various investigations on the properties of electric waves, one property has not yet attracted so much attention as it deserves—the action of long ether waves in modifying the molecular structure of matter. Apart from the interest attached to the relation between ether, electricity, and matter, the subject is of importance as affording not only a very important verification of the identity of visible and electric radiation, but also establishing the continuity of all radiation phenomena. These phenomena occupy the borderland between physics and chemistry, and their study may therefore be expected to throw much light on several subjects at present imperfectly understood. The study of the action of electricity and of ether waves on matter in the form of solids presents many difficulties, owing to the great complexity of atomic and molecular aggregation which characterises the solid state of matter. But the phenomena often met with in theory and practice is, unfortunately, in reference to matter in a solid state. The means of investigation are almost wanting: chemical tests give us no information, for they tell us (and that in a few cases only) of the ultimate change in the mass as a whole, and not of the protean transformations that are constantly taking place in it under the action of ever-varying changes in physical environments. In the following investigations the difficulties mentioned above were constantly present, and the attempts to meet them may therefore be of some interest.]

In my former paper† I described the contact-sensitiveness of various elementary substances to electric radiation. It was shown that though many substances exhibit a diminution of contact-resistance, there are others which show an increase of resistance—an increase which, in certain cases, lasts only during the impact of electric waves, the sensitive substance automatically recovering its original conductivity on the cessation of radiation. There are thus produced two opposite effects, either an increase or a diminution of resistance, depending on the nature of the substance.

The effect of increase of contact-resistance is not an exceptional or isolated phenomenon, but is as normal and definite under varied con-

* Matter in brackets [ ] added March, 1900.

† "On a Self-recovering Coherer, and the Study of the Cohering Action of Different Metals," 'Roy. Soc. Proc.,' vol. 65.

ditions as the diminution of resistance noticed in the case of iron filings. These two specifically different effects have to be recognised, and it would be advisable, to avoid misunderstanding, to use a simple term to indicate both these effects, and distinguish them from one another, by calling the one positive and the other negative. The term "coherence" applied to the normal *diminution* of resistance exhibited by certain metals by the action of electric waves cannot be applied in all cases, as there is, as has been said before, another class of substances which exhibits under *normal* conditions an *increase* of resistance. The term "decoherence" has been used to indicate the effect of mechanical tapping on fatigued substances of the former class; this produces an increase of resistance, and at the same time restores the sensitiveness. The action of tapping on fatigued specimens of the latter class is, however, a diminution of resistance.

I have shown in a former paper that the seat of sensitiveness is confined mainly to the surface layer of the sensitive substance, and that the nature of the substratum has little or no effect on the sensitiveness. Thus, a substance which exhibits a strong diminution of resistance, if coated with an extremely thin layer of a substance of the other class which shows an increase of resistance, will now exhibit an increase of resistance. It is seen that the action is one of the bounding layer or skin of the sensitive substance. There is a Sanskrit word, *twach*, which means the skin; and as the phenomenon dealt with in the present paper is one of sensitiveness of *twach*, I shall use the expression "electric touch" in the restricted sense of contact sensitiveness to electric stimulus, the touch being regarded as *positive* when electric oscillation produces an increase of conductivity or diminution of resistance, and *negative* when the contrary effect is produced. Substances which exhibit a decrease of resistance will be called positive, and those which show an increase will be regarded as negative. The above terms are to be regarded as convenient substitutes for long descriptive phrases.

The phenomenon of contact-sensitiveness seems at first to be extremely anomalous, and there appears to be little relation between substances which exhibit similar electric sensitiveness. Taking iron as an example of a very sensitive substance, it is seen to be easily oxidised, and from this it may be inferred that a slight oxidation on the surface is favourable for sensitiveness. This view obtains some support from the consideration that the so-called noble metals are not as sensitive as iron. But the metals nickel and cobalt, which are bright and not easily oxidised, are also very sensitive. The very sensitive metals iron, nickel, and cobalt are all magnetic, and it might be thought that magnetic property is favourable for electric sensitiveness, but a dial magnetic substance like bismuth is also found to exhibit a fairly strong sensitiveness. Again, from the strong diminution of resistance exhibited by magnesium, it may be inferred that the sensitiveness depends

on the electro-positive character of the metal; but potassium, one of the most electro-positive metals, exhibits an unusual increase of resistance, a property which it will be shown in a future paper it shares with some of the most electro-negative elements.

There is one property, however, which at first would seem to be in some way related to the sensitiveness of metals—the volatility of metals under the cathodic stimulus, investigated by Sir William Crookes,* who gives the following list of metals, arranged according to their volatility :—

Palladium	108	Copper	40
Gold	100	Cadmium	31·99
Silver..................	80·68	Nickel	10·99
Lead	75·04	Indium	10·49
Tin.....................	56·96	Iron	5·5
Brass	51·58	Magnesium	very slight
Platinum	44	Aluminium	very slight

In this list the substances which are most volatile, *e.g.*, Pd, Au, Ag, are not very sensitive, whereas Fe, Al, Mg, which are least volatile, are strongly sensitive. But the above series does not exactly coincide with the series of electric sensitiveness. Again, the volatility of platinum is retarded in hydrogen gas, but an experiment carried out to determine the sensitiveness of platinum in hydrogen failed to show any great increase in the sensitiveness.

None of the above suppositions give any satisfactory explanation of the numerous anomalies in the contact-sensitiveness of metals. It then appeared that the observed effect is not due to a single cause but to many causes. An observer studying the dilatation of a gas under reduced pressure, and ignorant of the effect of temperature, will doubtless encounter many anomalies. In the phenomena of contact-sensitiveness the variables are, however, far more numerous, and the different possible combinations are practically unlimited. It therefore became necessary, by a long and tedious process of successive elimination, to find out the causes which are instrumental in producing the observed effect; the results obtained throw some light on this intricate subject. The following are some of the principal directions in which a systematic inquiry was carried out :—

A. On the difference between mass action and molecular or atomic action, with reference to the phenomenon of contact-sensitiveness.

B. On the change of sign of response in a receiver due to a variation of the radiation intensity.

* Crookes, 'Roy. Soc. Proc.,' vol. 50.

C. On the physico-chemical changes produced in a sensitive substance by the action of electric radiation, and on the radiation-product.
D. The phenomena of electric reversal and of radio-molecular oscillation.
E. On "fatigue" and the action of mechanical tapping and other disturbances by which the sensitiveness of a fatigued receiver may be restored.
F. On the nature of the passage of electricity through imperfect contacts, and the influence of various physical agents on the current.
G. On the systematic study of the contact-sensitiveness exhibited by metals, non-metals, and metalloids.
H. On the contact-sensitiveness exhibited by alloys and compounds.

I intend to treat the above subjects in some detail, and in the present paper will especially deal with the first five lines of investigation. These, it is hoped, will afford an explanation of some of the most perplexing anomalies. All the subjects mentioned above are more or less interdependent, but their treatment in one paper would make the subject very complicated. It would be easier to take a more generalised and complete view of the subject as a whole, after each of the above-mentioned inquiries has been separately considered. With reference to the flow of electricity through imperfect contacts, I need only mention here that the phenomenon seldom obeys Ohm's law. In many instances the phenomenon appears more allied to the discharge of electricity through a gaseous medium.

Mass Action and Molecular Action.

Of the attempts made to explain the action of contact-sensitiveness, Professor Lodge's theory of coherence has been the most suggestive. The coalescence of water and mercury drops in Lord Rayleigh's experiments, and Professor Lodge's observation of the welding of two metallic spheres by powerful oscillatory discharge in the neighbourhood, apparently lend much support to the theory of electric welding, which explains in a simple manner the diminution of contact-resistance of various metallic filings when subjected to strong electric variation.

On this theory it follows that *all* imperfect contacts should exhibit a diminution of resistance when subjected to electric radiation. In carrying out a systematic investigation of the contact-sensitiveness of metals, I, however, found that there are substances, of which potassium may be taken as a type, which exhibit an increase of resistance. Potassium is not a solitary instance; I have found a large number of elements exhibiting this action; the number of compounds which exhibits a similar action is also considerable. Other experiments will

be presently described which would show that the theory of coherence is inadequate. From the above it would appear that the subject is far more complex than was at first supposed. For various reasons it would be best to distinguish between the two different actions, which may conveniently be described as mass action and molecular action.

Mass Action.—By this is meant the general action, say, between two masses when placed in a very strong electric field. Under the given circumstance, sparking may take place between the bodies, and the two may thus be welded together. From what has been said it will be seen that such action is *non-discriminative*—that is to say, the action will be the same whatever the chemical or physical nature of the substance may be. The best way of showing this action is with drops of liquid, with surface contamination, for any incipient welding will be at once exhibited by the complete coalescence of the drops. The non-discriminative nature of the action is shown in a striking manner in the following experiment. I may mention here that fragments of solid potassium, and in a lesser degree sodium, exhibit an increase of contact-resistance under the action of electric waves. I made a liquid alloy of potassium and sodium, and drops of this alloy were allowed to float on the stratum separating dense Rangoon oil from lighter kerosine, the alloy being of an intermediate density. The drops coalesced when placed in an intense alternating electric field. The next experiment was made with potassium heated under melted (hard) paraffin. By stirring the molten K with a glass rod, the metal was broken up into numerous spherical drops. These also coalesced under similar electric influence. It is, however, to be borne in mind (1) that in the above experiment the substance is in the form of a liquid, and that in this particular condition certain important molecular changes, to be presently described, cannot very well take place; (2) that the conditions of the experiment are abnormal.

Experiments will be presently described which will show that the observed variation of conductivity produced by radiation is not due to coherence, but to certain molecular changes of an allotropic nature.

Molecular Action.—By this I mean the allotropic modification produced in a substance by the action of electric waves, the allotropic change being due to a difference in the atomic or molecular aggregation. It will be shown that such molecular change does take place by the action of electric waves, and that all the observed effects of variation of resistance of the sensitive substance may be explained on the theory of allotropic transformation due to the above cause. The effect due to molecular changes in a substance is also expected to be modified by the chemical nature of the substance; thus the molecular action due to radiation, giving rise ultimately to the variation of conductivity of the sensitive substance, will be *discriminative*, in contradistinction to the non-discriminative mass action.

Is the effect of radiation due to non-discriminative coherer action or to the discriminative molecular action ? That the effect is discriminative, and therefore molecular, appears to be decisively proved by the experiments described below. If further proofs are necessary, they are afforded by the characteristic curves of variation of current with the E.M.F. given by the three types of substances, positive, negative, and neutral; by the continuity of radiation effect on matter; and, lastly, by certain remarkable results I obtained, which show that the effect of ether waves on elementary substances depends on the chemical nature of the substance; in other words, the effect is found to be a periodic function of the atomic weight of the substance. A detailed account of the above will be given in a future paper.

On the Change of Sign of Response in the Receiver, due to a Variation of Intensity of Radiation.

After finding the increase of resistance exhibited by certain substances, I wished to see whether these showed any further difference as compared to substances which exhibit a diminution of resistance. In my determination of the "Index of Refraction of Sulphur for the Electric Ray,"* I used the method of total reflection. I often noticed that just before total reflection, when the intensity of the transmitted beam became comparatively feeble, the receiver indicated an increase instead of the usual diminution of resistance. Professor Lodge mentions in one of his papers that an iron filing coherer exhibits an increase of resistance when acted on by feeble radiation. Thus, if positive substances like iron give a negative reaction, with the diminution of radiation intensity, negative substances may be expected to give a positive reaction with feeble radiation. Very sensitive substances are, however, not so well adapted for an exhibition of this reversed action, possibly because the range of sensibility is comparatively great. But it is not difficult to demonstrate this property in the case of moderately sensitive substances.

The following experiment with a moderately negative substance (arsenic) and a moderately positive substance (osmium) will bring out this interesting peculiarity in a clear manner. The intensity of incident radiation may be varied in two ways—(1) by removing the radiator further and further from the receiver, or (2) by using polarised radiation, whose intensity may be varied by the rotation of an analyser. In the experiment to be described, the first method was adopted.

Experiments with Arsenic Receiver.—A receiver was made of freshly-powdered arsenic. The radiator used emitted radiation of strong intensity. It was at first placed close to the receiver, and there was produced a moderate increase of resistance. It was then removed

* 'Roy. Soc. Proc.,' 1895.

further and further, and the increase of resistance became less and less. When the distance was increased to 25 cm. the action was reduced to zero. When the distance was increased to 30 cm. there was a diminution of resistance, showing that 25 cm. is, in this case, the *critical* distance. The receiver continued to exhibit a diminution until the radiator was removed to a distance of 70 cm., when the radiation intensity was too feeble to affect the receiver. Now this critical distance may approximately be regarded as a measure of the sensibility of the substance. In this particular case the electric touch has a negative sign. If by any means (some of which will be described later on) the substance becomes more sensitive, *i.e.*, more negative, the critical distance will be increased. On the contrary, if the sensitiveness becomes less (the substance tending towards positive direction) the critical distance will be decreased. The application of this principle is of importance as affording a means of determining the variation of sensitiveness under different conditions.

Experiments with Osmium Receiver.—Substances which are feebly positive give a diminution of resistance when the radiator is close to the receiver, and an increase of resistance when the radiator is beyond the critical distance. Thus the critical distance for an osmium receiver (whose normal action is moderately positive) was found to be about 250 cm. The radiator at a distance of 300 cm. produced a deflection of − 3 divisions in a galvanometer placed in the receiver circuit. But at the distance of 200 cm. the deflection became + 4, and at the reduced distance of 50 cm. the deflection became + 150 divisions.

In order to avoid confusion, we may choose to call the effect due to strong intensity of radiation as the normal action. The sign of normal action might further be verified, wherever possible, by obtaining a reverse action with feeble radiation intensity.

Molecular Changes produced in Matter by the Action of Electric Waves.

A sensitive receiver made, say, of iron powder, has its conductivity suddenly increased by the action of electric radiation; but the sensitiveness of the receiver is lost after the first response, and it is necessary to tap it to restore the sensitiveness. On the theory of coherence, the loss of sensitiveness is explained by supposing that electric radiation brings the particles nearer and welds them together, and that the sensitiveness can then only be restored by the mechanical separation of the particles. This supposition, however, fails in the case of substances which exhibit an increase of resistance by the action of radiation. It may, however, be supposed that by some process, little understood, the particles are slightly separated by the action of electric waves, thus producing the observed increase of resistance. On this view, however, the restoration of sensitiveness by tapping remains unexplained.

Again, if the increase of resistance is due to a slight separation of particles, suitable small increase of pressure ought to restore the original conductivity, as also the sensitiveness. It is, however, found that a considerable pressure is required to restore the original current, as if the outer layers of the particles were rendered partially non-conducting by radiation, and had to be broken through before the original current could be re-established. It is also found that though the sensitiveness is restored by this expedient of increasing the pressure, yet the restoration is only partial, and that after a repetition of this process the receiver loses its sensitiveness almost completely.

I have attempted to find out an explanation of this obscure "fatigue" effect. This subject will best be treated in connection with the anomalous behaviour of silver, which I find is also in a manner connected with the fatigue effect. Silver, when subjected to radiation, exhibits, as indicated in my last paper, sometimes an increase, and at other times a decrease, of resistance. The difficulty in this case cannot be explained on the supposition of variations of radiation intensity, as the anomaly persists even when the intensity of radiation is maintained uniform by keeping the radiator at a fixed distance.

In order to explain these actions, I assumed the following hypotheses, which, with the necessary deductions, are given below :—

(1) That electric radiation produces molecular change or allotropic modification in a substance.

(2) That, starting from the original molecular condition A, the effect of radiation is to convert it, to a more or less extent, into the allotropic modification B (the latter condition will be designated as the "radiation product"). It follows that this change from one state to the other must be accompanied by a corresponding change in the physical properties of the substance.

(3) As one of the properties of a substance is its electric conductivity, any allotropic changes produced by radiation should be capable of being detected by a variation in the conductivity of the substance.

(4) As a molecular strain is produced during transformation from A to B, at a certain stage there may be a rebound towards the original state A. Thus, after the molecular change from A to B condition has reached a maximum value, the further action of radiation may be to reconvert, to a more or less extent, B to A, this reversal of effect being indicated (see No. 3) by a corresponding electric reversal.

(5) That the ultimate loss of sensitiveness, known as "fatigue," is due to the presence of the radiation product, or strained B variety, along with the A variety, the opposite effects produced by the two varieties neutralising each other.

The justification for the above hypotheses is to be sought for—

(1) From analogy with other known radiation phenomena.

(2) From experimental proof—

(*a*) Of the allotropic transformation being attended with changes in the conductivity of the substance.

(*b*) Of the existence (and, if possible, the production by chemical means) of an allotropic modification analogous to the radiation product or B variety, whose reaction should be opposite to that of the substance in a normal condition (A variety).

(*c*) Of the assumption that after the maximum transformation of A into B, the further action of radiation is to reconvert, to a more or less extent, the form B into A, such transformations giving rise to electric reversals.

(*d*) Of the existence of the radiation product in a fatigued specimen.

The above mentioned hypotheses will obtain still stronger support if further deductions from the above theory are borne out by confirmatory experiments.

I will now describe investigations on the lines sketched above.

Allotropic Modification produced by Visible Radiation.

As regards the action of radiation in producing allotropic modification, several such instances are known in the case of visible radiation. In the familiar example of the conversion of yellow phosphorus into the red amorphous variety, the effect is quite apparent. But this is not the case in the transformation of the soluble sulphur into an insoluble variety by the action of light; here the transformation is not apparent, and has to be indirectly inferred from the insolubility of the solarised product in carbon bisulphide. The reason why a far larger number of instances of allotropic transformation produced by light is not known is not because such effects are not more numerous, but because we are unable to detect such changes. It must be admitted that our knowledge of molecular changes, specially in a solid, and the means of their detection, is at present extremely limited.

Variation of Conductivity produced by Allotropic Changes.

There is one method of detecting these molecular variations to which little attention has hitherto been given, but which appears to be of great interest, and promises to yield important results in investigations of this class. It is evident that changes in molecular structure must be attended with changes of physical properties, electric conductivity being one of them. Among other instances of allotropic changes attended with changes in electric conductivity may be mentioned the wide difference of conducting power between graphite and diamond. The same great differences of conductivity is seen between the crystalline and amorphous varieties of silicon, and between the "metallic"

and white varieties of phosphorus. But it is not at all necessary to take only such extreme cases to show the influence of molecular or atomic aggregation in influencing the conductivity. This effect is brought into painful prominence by the variation produced in spite of all precautions in our standards of resistance.

Experimental Proof of Allotropic Changes being attended with Variation of Conductivity.—I shall now describe a direct experiment by which the change of conductivity produced in a substance by molecular change is exhibited. Red mercuric iodide is converted into the yellow variety by the application of heat, and the substance does not return to its original state till after a considerable lapse of time. The recovery here is very slow. A small quantity of mercuric iodide was now placed in a tube provided with sliding electrodes, and a current was made to pass through the substance by suitable compression. The conductivity of the substance is rather small, and therefore a thin stratum should be taken for experiment. The current is observed by means of a delicate galvanometer. On the application of heat to the tube (which converts the red into the yellow variety), there was at once produced, simultaneously with the molecular transformation, an increase of conductivity. This effect is not due to a rise of temperature, for the increased conductivity was still exhibited on cooling the tube. From this experiment it is seen that the molecular changes can be inferred from changes in the conductivity. In the case described above, the recovery from the B, or second stage, to the first stage, A, is slow; but there may be substances (and there are such substances) where, under the given conditions of temperature and other physical surroundings, the first stage is far more stable than the second; the substance will then pass back quickly from the B condition to the primitive state, on the cessation of the exciting cause, which gave rise to the transient B effect. The substance will in this case be "self-recovering."

[*Electrical Reversal in the Radiation Product.*

In the hypotheses given above, it was said that the reaction of the radiation product, or B variety, should be opposite to that of the substance in the normal condition, or in the A state. Thus a negative substance which by the action of radiation shows an increase of resistance during conversion from the A to the B state should exhibit a diminution of resistance when B variety is acted on by electric waves. The contrary would be the case with positive substances.

The following tabulated statement indicates the phenomena exhibited by two classes of substances :—

Sign of electric touch.	Action of radiation on the fresh or A variety of the substance.	Further action of radiation on the radiation product or B variety.
Positive, *e.g.*, iron......	Diminution of resistance.	Increase of resistance.
Negative, *e.g.*, arsenic...	Increase of resistance.	Diminution of resistance.

We have thus two distinct classes of phenomena dependent on the sign of electric touch. If K_A represents the conductivity of the fresh substance, and K_B the conductivity of the radiation product, then

(1) With positive substances, as the conductivity of the radiation product is greater ($K_B > K_A$), the first action of radiation would be to produce a diminution of resistance. This diminution will continue to be exhibited till the maximum amount of B variety is produced. The further action of radiation now will be to reconvert B into A; but as $K_A < K_B$ there would now be produced a diminution of conductivity, and a galvanometer in circuit will indicate an electrical reversal. The reconverted A variety may again be transformed to a greater or less extent to B, and in this way a series of reversals may take place, due to the continued action of radiation producing oscillation in molecular or atomic groupings. I shall designate this as the phenomenon of radio-molecular oscillation.

(2) With negative substances the conductivity of the radiation product is less ($K \ < K_A$), and the first action of radiation will therefore be an increase of resistance. The phenomena exhibited by these negative substances will precisely be opposite to those shown by the positive substances.

The above is but an approximate representation of the phenomena. To be more accurate, one has to take into account the partial changes and the effect of radiation on these changed products. Thus, at first suppose the substance to be entirely made up of A variety (this would rarely be the case). The first flash of radiation converts a large portion of A into B, the substance now being a mixture of A and B. The action of the next flash would be to convert the unchanged A into B, and reconvert to a more or less extent B into A. The electric response will thus be very strong at the beginning, but will become continuously less and less. When the proportion of B has attained a maximum value, the reconversion of B into A will become relatively large, and thus give rise to reversal effect.

I spoke of the conversion "to a greater or less extent" of one variety into the other. There is also the question of the relative stability of the two varieties under the given conditions. From the above it will be seen what possibilities there are in the way of different combinations, and the varied phenomena thereby rendered possible. I will presently describe some of the typical cases.

In the above it has been assumed that the reaction of B variety is opposite to that of A. As previously mentioned, in working with a silver receiver I found it, when fresh, exhibiting at first a diminution and, subsequently, an increase of resistance. The anomalous action may be explained by supposing the normal fresh silver Ag to be positive, and the radiation product Ag′ negative. These two varieties would thus give rise to opposite reactions. To justify the assumptions made above, it became necessary to obtain by some other means a variety of silver Ag′, analogous to the hypothetical negative variety.]

Two Varieties of Silver.

After many unsuccessful attempts, I at last obtained a variety of silver which gives a moderately negative reaction (increase of resistance). Silver chloride was first precipitated by the addition of dilute hydrochloric acid to silver nitrate solution. The precipitate was then reduced to metallic silver by zinc filings, the excess of zinc being dissolved off by the action of HCl. This form of silver gives a negative reaction. Direct precipitation of silver produced by dipping a piece of zinc in $AgNO_3$ solution gives a positive variety. The negative product Ag′ is perhaps better formed at relatively low temperatures, for the products obtained on certain warm days, the thermometer registering 27° C., were very feebly negative, and passed into the positive state after an interval of twenty-four hours. But on cold days (temperature = 22° C.) the products obtained were stable. I have specimens which have kept the negative property unimpaired for nearly three months. The negative property is not due to any accidental impurity, for pure silver obtained by Stas' method also gave the negative reaction. The negative reaction may, however, be supposed to be due to a thin film of chloride formed during reduction. I washed the Ag′ with NH_3, then with water, and, after drying, the result was still a negative reaction. I then carried out a parallel set of experiments with ordinary silver filings. Two separate quantities were taken; one was shaken with only HCl, the other was mixed with zinc filings, and the excess of zinc was dissolved off with HCl; the two specimens were then washed and dried. Both gave the positive reaction of ordinary silver. The above experiments are interesting for the production, by chemical means, of an allotropic variety analogous to the transitory radiation product.

There are other differences of electric behaviour between Ag and Ag′; for instance, when made into a voltaic cell, the two varieties give a P.D. of about 0·12 volt. There are other interesting peculiarities about this cell, the consideration of which is postponed to a future occasion.

Electric Reversal.

It now remains to be proved that the "radiation product" exhibits a change of sign of electric touch. The sensitiveness of certain substances belonging to each of these two classes is very great. On the other hand, in the transition from one class to the other, substances are met with the sensitiveness of which is rather feeble. The experimental verification of the hypotheses mentioned above seemed at first very difficult, as the reversed action was likely to be masked by the stronger normal action of the still unconverted portion of the substance. It however occurred to me that if slightly sensitive substances were taken, then the direct and reversed actions were likely to be obtained with less difficulty. For this reason I took for my first experiments arsenic, which is moderately negative. It is however possible, though the adjustments are difficult, to exhibit the reversed actions even with strongly sensitive substances, and as a type of such actions that of iron will be taken as an example.

Observations with Arsenic Receiver.—Experiment I.—A receiver was made with freshly powdered arsenic; the critical distance was found to be 25 cm.; that is to say, when the radiator was moved from 1 to 25 cm. there was always produced an increase of resistance, while beyond this distance there was a diminution of resistance; the critical distance, 25 cm., may therefore be taken as an approximate measure of the negative character of the specimen. As has been said before, if through any cause the substance becomes more negative, the critical distance will be increased; but if the substance tends towards the positive direction by becoming less negative, then the critical distance will be decreased. The receiver was now continuously subjected to radiation for ten minutes. After this it was found that the receiver gave a diminution or positive reaction, even when the radiator was brought close to the receiver. The action of radiation has thus reversed the sign of electric touch.

Experiment II.—Any arbitrary length of exposure labours under the defect that what is observed is the final effect, the intermediate effects not being taken into account. In order to observe the intermediate effects, a very laborious series of observations is necessary. The experiment was therefore modified in the following manner:—A fresh receiver was subjected to radiation, and observations at intervals of fifteen seconds were taken to test the nature of reaction of the sensitive substance. The first action of radiation on the fresh specimen was a great increase of resistance—so very great that the current was reduced to zero; it was no longer possible to make any further observation without re-establishing the current. This was done by a very gradual increase of pressure, effected by means of a fine micrometer screw which moved the compressing electrode in a perfectly parallel manner. There

should be no jarring motion, as mechanical disturbance was found to break up the complex atomic aggregation in the radiation product. During eight minutes of exposure the receiver continued to exhibit an increase of resistance, after which the substance became positive, being converted to the B state; this positive state lasted for a minute under exposure to radiation, then there was a reversal to the original negative state—as if the structure so laboriously built up suddenly gave way. Subsequently there were series of reversals, the specimen becoming more and more inert, and, after an exposure of about thirty minutes, the sensitiveness was practically lost.

The curve given below represents approximately the results of the experiment. During certain periods the substance became so nearly neutral that it was difficult to interpret whether the substance was positive or negative. The lower halves of the curve represent the negative and the upper halves the positive states, and the corresponding numbers represent, in minutes, the duration of these states.

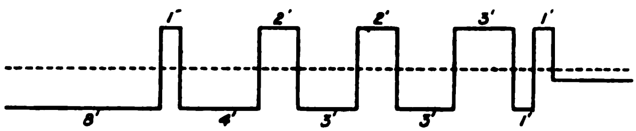


FIG. 1.—Electric Reversal Curve.

[*Radio-molecular Oscillation.*

It was said that, owing to molecular reversals due to radiation, there should be a corresponding series of electric reversals. In this investigation it is essential that the substance examined should be completely protected from all disturbances, such as mechanical vibration, &c., and only subjected to the action of radiation. The experimental difficulties are very great. If we take a strongly sensitive positive substance—say iron, the effect of the first flash of radiation (a diminution of resistance) is very great, and the subsequent relatively feeble reversal effects, unless carefully looked for, are liable to pass unnoticed. After the first adjustment, the receiver should on no account be touched, as mechanical jars are found to undo the effect of radiation. Though by special care the mechanical jars could be reduced to a minimum, yet it appeared advisable to devise appropriate means by which the necessity of touching the receiver for subsequent adjustments is altogether avoided. The method adopted to this end varies with individual cases. In the case of arsenic, for example, the action of radiation is often to produce a very great increase of resistance, and thus convert the substance into a non-conductor. The pressure has therefore to be so adjusted at the beginning, that the substance can never become altogether non-conducting; the

receiver is thus absolutely free from all effects, except those which are to be observed—viz., the effects due to continuous action of radiation. The time of exposure is accurately measured by counting the individual flashes of radiation, due to interruption of the primary current in the Ruhmkorff coil by a tapping key. The conductivity of the substance at a given moment is inferred from the deflection of the galvanometer in circuit with the receiver. When feeble radiation is used, it takes an inconveniently long time to obtain reversals; there is, besides, a tendency of self-recovery in the receiver. In order to expedite the reversals, the incident electric radiation is made very strong.

Before entering into the detail of the results obtained, I will say a few words about the principal types likely to be met with. We may have the following:—

I. Substances in which the B state is unstable under the given conditions; the B state will therefore only persist during the action of radiation, the substance relapsing into the original condition on the cessation of radiation. Two cases are possible (i) when the substance is positive, (ii) when the substance is negative.

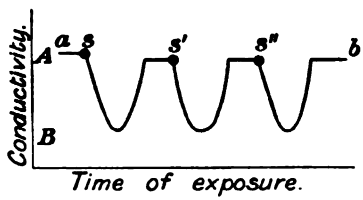


FIG. 2.—Curve for Potassium.

The latter case is exemplified by potassium. In the above curve (fig. 2), A and B represent the two molecular states. The substance being negative, A is more conducting; *a* represents the conductivity of the fresh specimen; the thick dots S S′ . . . the individual flashes. It is seen that the effect of radiation is to produce a sudden diminution of conductivity, due to the transient formation of B variety with its diminished conductivity. The substance, electrically speaking, is highly elastic, and the limit of its elasticity is also very great. With the majority of substances, however, self-recovery is only possible when the narrow limit of elasticity is not exceeded.

II. In this class the radiation product is somewhat stable; the successive conversions from A to B and from B back to A are supposed to be complete. Probably there is no substance which exhibits this action in a perfect manner, but we have an approximation to this condition in the case of magnesium, which under proper adjustments shows successive complete reversals for a long time. The substance, however, after a time exhibits the effect of fatigue.

The curve given in fig. 3 clearly exhibits the reversals. This curve also explains the behaviour of magnesium noticed in my last paper, which appeared very curious at the time. "It is sometimes possible to so adjust matters that one flash of radiation produces a diminution of resistance, and the very next flash an increase of resistance. Thus a series of flashes may be made to produce alternate throws of the galvanometer needle."*

The receiver was so adjusted as to give a deflection of five divisions.

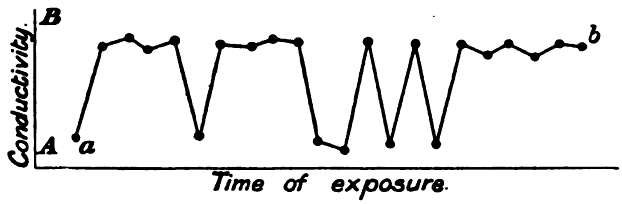


FIG. 3.—Radio-molecular Oscillation Curve for Magnesium.

The first flash of radiation produced an increased deflection of 90 divisions (magnesium having a positive electric touch); the second flash produced a further deflection of five divisions, the third flash produced a *negative* deflection of five divisions, the fourth flash produced + 5, the fifth flash gave − 90 divisions, and the sixth flash a deflection of + 90. The reversals followed each other almost regularly, till the substance became insensitive.

III. Lastly, we may have a class of substances where the conversion from one state to the other is not complete. Here, again, we get two sub-divisions, owing to the distinction between positive and negative substances.

Taking first the case of a positive substance (see fig. 4 (β)), the original conductivity of which is represented by a, the action of the first few flashes of radiation would be to produce a great increase of conductivity by the formation of B variety; the next flashes convert

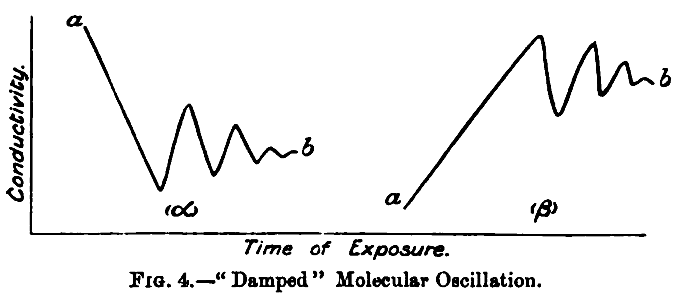


FIG. 4.—"Damped" Molecular Oscillation.

* "On a Self-recovering Coherer," &c., 'Roy. Soc. Proc.,' vol. 65, p. 169.
2 0 2

B back to A, but not completely, and the negative deflection will be less than the previous positive deflection. Owing to this "damping" effect, the oscillation curve will approximate to a logarithmic decrement curve. After a series of reversals the oscillation dies away, and the substance becomes almost inert. A glance at the hypothetical curve to the right shows that at the inert stage, *b*, *the substance as a whole ought to become more conducting the fresh specimen*, *a*.

The opposite should be the case with negative substances (see fig. 4 (α).

Fig. 5 exhibits the actual curve obtained with a (compound) positive substance.

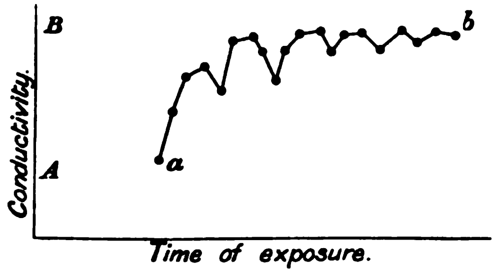


Fig. 5.—" Damped " Oscillation Curve for a Positive Substance.

It is remarkable for its regularity. The next figure (fig. 6) gives the curve for iron. The first diminution of resistance is too great to be properly represented in the diagram. Here we have the same type as in the previous case; *the inert stage*, *b*, *is also more conducting than a*.

IV. I will now consider the case of a negative substance exhibiting damping; arsenic will be taken as an example where the damping is not so great as in the case of iron.

Fig. 7 represents the actual curve obtained with arsenic (compare with the hypothetical curve for a negative substance, fig. 4 (α)).

It will be observed that *the substance in the fatigued state is, on the whole, less conducting than in the fresh condition*, as we were led to expect from the hypothetical curve. It will also be seen that the oscillations are very regular towards the end. The curves given in figs. 6 and 7 are those obtained with specimens immediately after they were set up. Had I allowed them a period of rest to allow the particles to get properly settled, I would have got curves even more regular. It is, however, evident that in substances exhibiting damping, two opposite electric conditions are induced in fatigued specimens; the positive becomes on the whole more conducting and the negative less conducting than in the fresh specimens. At the inert stage the rate of mutual conversion from one state to the other probably becomes equal, and the apparent fatigue is thus not due to the absolute want of sensi-

tiveness of the constituent varieties, but to the opposite reactions of A and B balancing each other.

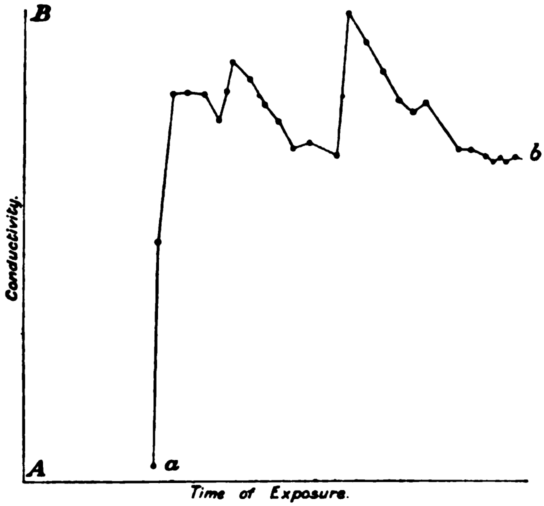


Fig. 6.—Curve for Iron.

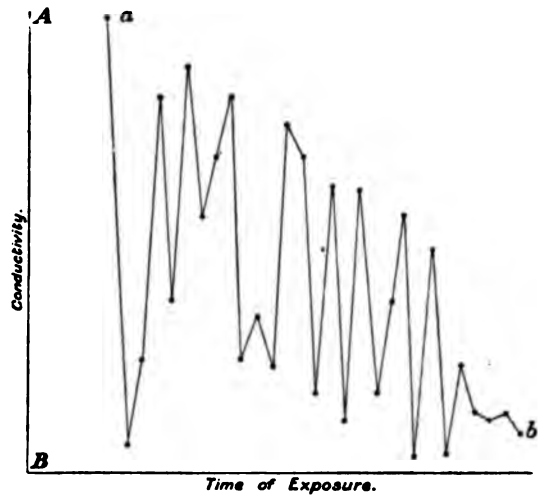


Fig. 7.—Curve for Arsenic.

We may now apply some further crucial tests to verify the suppositions made above.

Restoration of Sensitiveness to a Fatigued Substance.

It was said that the inertness of the substance, after long exposure, is due to the presence of a relatively large amount of strained B variety. It therefore follows that if by any means we can transform B into A, then after such a transformation there ought to be a restoration of the sensitiveness. It has also been stated that the B variety under ordinary circumstances is less stable than A. If now we apply a disturbing cause which is unilateral in its action—that is to say, if it converts B into A and not A into B—then such a disturbing cause will resensitise the fatigued substance.

Effect of Mechanical Disturbance.—Of the unilateral actions, mechanical vibration is one; for it is known that by the action of friction a substance may pass from one modification to another in one direction only. Thus the change of monoclinic into rhombic variety of sulphur is hastened by scratching with a glass rod, but the change does not take place in the opposite direction. We may now apply the crucial tests. Mechanical vibration will transform B into A, and with positive fatigued substances this ought to produce an *increase* of resistance (as A is less conducting than B); with negative substances the same disturbance ought to produce a *diminution* of resistance.

Effect of Heat.—There are other methods by which the B variety may be transformed into A; the more subtle molecular disturbance due to heat may be expected to be even more effective in producing the transformation. Here, too, the crucial test is that by slight heating the fatigued positive substance ought to show an increase of resistance, and the negative substance a diminution of resistance. The two following curves (figs. 8 and 9) confirm in a remarkable manner my anticipations.

Effect of Heat and Mechanical Disturbance on a Positive Fatigued Substance.—I shall at first deal with the curve for iron. At the end of No. 6 curve, the substance was left in the inert stage *b*. While in this state, the receiver was heated to a slight extent. Observe in the dotted portion of the curve the sudden fall of conductivity (increase of resistance). I should like to say here, that, though the fall has been indicated by a straight line, as representing the somewhat sudden fall of conductivity, I sometimes noticed on careful inspection a slight oscillatory movement of the galvanometer spot during this process. The significance of this I will notice on a future occasion. The ultimate effect of slight heating (excess of heat produces other complications) is the restoration of the original reduced conductivity. If the application of heat transforms B into A, we may expect the substance to regain its sensitiveness, which it lost in the fatigued stage *b*. The receiver was now exposed to radiation, and it at once responded, exhibiting almost its original sensibility. Observe how the subsequent

portion of the curve is a repetition of the curve No. 6, and how the substance arrives at the second fatigued state b'. To observe the

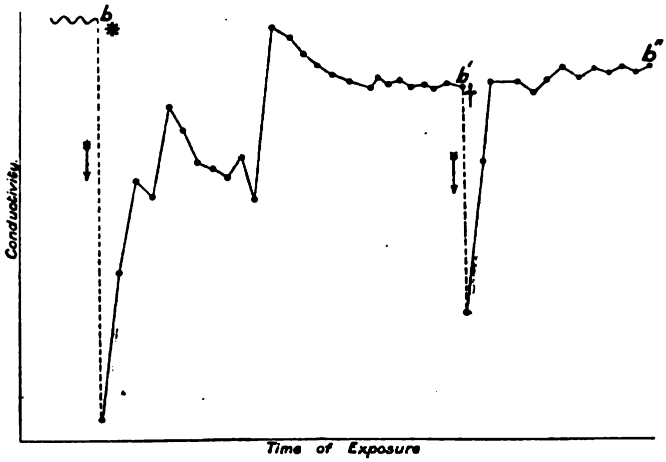


FIG. 8.—Curve showing the Effect of Heat and Mechanical Vibration on a fatigued Iron-filings Receiver.

* Application of heat. † Application of a tap.

effect of mechanical disturbance a gentle tap was given to the receiver, and at once there was produced an increase of resistance due to the transformation of B into A, the receiver regaining its sensitiveness by the transformation. The action of radiation was continued, and after a few reversals the substance once more arrived at the third fatigued state, b''. The process described above could be repeated any number of times.

Effect of Heat and Mechanical Disturbance on a Negative Fatigued Substance.

Experiments similar to the above were carried out with an arsenic receiver. From the curve given below (fig. 9) it is seen that the reaction of the negative substance is in every respect opposite to that of a positive substance. It will be noticed that the same cause—*i.e.*, heating or tapping—produces, as necessary consequences of the hypotheses previously stated, two opposite reactions in the two classes of substance. I have been able to verify this deduction by observations with nearly a dozen different substances, and have not, so far, come across any to contradict it. It thus appears that tapping restores the sensitiveness not by the separation of the electrically-welded particles (in which case tapping ought to have produced an increase of resist-

ance in *both* the classes of fatigued substance), but by removing the strain in B and thus converting it into A.

The effect of electric radiation is thus to produce rearrangement of atoms and molecules in a substance; so does light produce new atomic and molecular aggregation in a photographic plate—a subject to be dealt with in detail in a future paper. Some of my audience at the Royal Institution (January, 1897) may remember my attempt at explaining the action of the so-called coherers (which, perhaps, may be better described as "molecular receivers") by analogy with the photographic action. I had then no proofs for the assertion. I have since been able to obtain experimental evidence that the two phenomena are

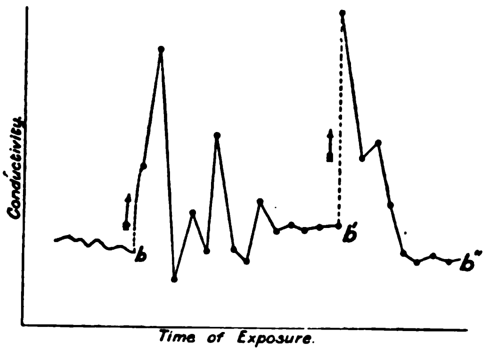


FIG. 9.—Curve showing the Effect of Heat and Mechanical Vibration on a Fatigued Arsenic Receiver.

b Effect of heat. *b'* Effect of tapping.

identical. The coherer may therefore be regarded as a linear photographic plate; since we are more likely to understand the complex photographic action from the consideration of the much simpler action of electric radiation on elementary substances, where the effects are not complicated by secondary reactions, a photographic plate may be regarded as merely an assemblage of "molecular receivers." I hope also to prove that nearly all the detectors of radiation are molecular receivers in reality. The investigation of this aspect of the subject has given me some extraordinary results; they seem to connect together many phenomena which at first sight do not seem to have anything in common. Another interesting question, the consideration of which has for the present to be postponed, is, Why is it that the sensitiveness is so marked in discontinuous metallic particles? In other words, Why is the phenomenon mainly one of skin or *touch*? Is the phenomenon wholly unknown in continuous solids?

The experiments described in the present paper show :—

(1) That ether waves produce molecular changes in matter.

(2) That the molecular or allotropic changes are attended with changes of electric conductivity, and this explains the action of the so-called coherers.

(3) That there are two classes of substances, positive and negative, which exhibit opposite variations of conductivity under the action of radiation.

(4) That the production of a particular allotropic modification depends on the intensity and duration of incident electric radiation.

(5) That the continuous action of radiation produces oscillatory changes in the molecular structure.

(6) That these periodic changes are evidenced by the corresponding electric reversals.

(7) That the "fatigue" is due to the presence of the "radiation product," or strained B variety.

(8) That by means of mechanical disturbance or heat, the strained product can be transformed into the normal form, and the sensitiveness may thereby be restored.

The method described above of detecting molecular changes is extraordinarily delicate, and is full of promise in many lines of inquiry in molecular physics. It is also seen that the phenomenon of contact sensitiveness, contrary to previous suppositions, is perfectly regular. There is no capriciousness in the response of sensitive substances to the external stimuli, which may be mechanical, thermal, or electric. The curves given above show it; but they fail to give a fair idea of the richness and variety of the molecular phenomena, seen as it were reflected in the fluttering galvanometer spot of light; of the transitory variations, of the curious molecular hesitation at critical times as to the choice of the structure to be adopted, and of the molecular inertia by which the newly-formed structure is carried beyond the position of stability, and the subsequent creeping back to the more stable position. The varieties of phenomena are unlimited, for we have in each substance to take account of the peculiarity of its chemical constitution, the nature of its response to ether waves, the lag and molecular viscosity. All these combined give to each substance its peculiar characteristic curve; it is not unlikely that these curves may give us much information as to the chemical nature and the physical condition of the different substances. I am at present trying to arrange an apparatus which will, by means of the pulsating galvanometer spot of light, automatically record the various molecular transformations caused by the action of external forces.

Before concluding, I take this opportunity of expressing my grateful acknowledgments to the Royal Society for the encouragement

received from the Society for the last five years during which investigations on Electric Radiation have been in progress at the Presidency College. I may say that the difficulties have been very numerous and disheartening, and that without this encouragement the work which it has been my good fortune to carry out would in all probability have remained unaccomplished. The Government of Bengal has also been pleased to evince a generous interest in these investigations. My assistant, Mr. Jagadindu Ray, and my pupils, Messrs. P. K. Sen, B.A., and B. C. Sen, B.A., have rendered me active assistance.]

"Contributions to the Comparative Anatomy of the Mammalian Eye, chiefly based on Ophthalmoscopic Examination." By GEORGE LINDSAY JOHNSON, M.D., F.R.C.S. Communicated by HANS GADOW, F.R.S. Received May 7,—Read May 17, 1900.

(Abstract.)

Observations were made on the eye of the living animal, 181 different species being examined, and frequently several individuals of the same species. The species comprise representatives of all the Mammalian orders except the Cetacea and Sirenia.

The conclusions arrived at can be summed up as follows:—

The *colour of the Fundus oculi* in animals devoid of a Tapetum is mainly determined by reflection from the choroidal pigment; in those with a Tapetum cellulosum (Carnivores) by the colour of the retinal pigment; in those with a Tapetum fibrosum (Ungulates) by the structural colour of the Tapetum modified by the colour of the retinal pigment. All the animals examined may be classed under three types —red, yellow, and green.

The *vascularisation of the retina* can be summarised as follows:—

1. Indirect supply by means of osmosis from the vessels of neighbouring parts. A. Hyaloid supply. (*a*) The corpus vitreum is nourished by a processus falciformis, the hyaloid vessels lying well inside the corpus vitreum (Elasmobranchs). (*b*) The hyaloid vessels spread over the surface of the corpus vitreum, being in consequence in the immediate vicinity of the retina (*e.g.*, holosteus and many teleosteous fishes). Hereto belong also the Amphibia and most of the Reptiles devoid of a pecten. B. Choroidal supply. This is probably the chief supply of the retina in those animals which possess a well-developed pecten (most Sauropsida), but are devoid of superficial hyaloid vessels. This choroidal supply by osmosis is also with certainty demonstrated in the Mammalia for at least part of the thickness of the retina.

2. Direct supply. A. From the superficial hyaloid vessels. This is

known to be the case when the hyaloid vessels are directly continued into the retina, where they produce two vascular layers. B. From special retinal vessels cumulating in the art. centralis. This mode is restricted to the Mammalia and some of the Snakes.

The vessels of the falciform process of the fishes and the central hyaline artery, wherever this occurs, are essentially the same. The falciform process and the pecten are analogous, but not homologous, structures. In Reptiles and Birds the hyaloid artery is superseded by a new development, viz., the Pectinal system. In some of the lower Mammalia both systems actually occur side by side, but both are rendered unnecessary by the development of a third system of supply, viz., special retinal vessels, which ultimately culminate in the possession of an art. and vena centralis retinæ.

Some of the normal conditions observable in certain animals closely resemble those which we find in Man as congenital defects or vestigial relics. 1. Membrana nictitans. A fully developed nictitating membrane active enough to sweep the whole cornea, exists only in the Ungulata, and not even throughout this order. In the Carnivora and Marsupials it is much less developed, whilst throughout the Primates, Rodents, Edentata, and Echidna it is still more reduced, and, with rare exceptions, entirely without movement. The primary use of this third lid, viz., that of cleaning the corneal surface, is lost within the class of the Mammalia, and seems to serve chiefly to protect the eye in the animals which graze and poke their heads down into the long and sharp grass. 2. The retractor muscle of the eyeball is of frequent occurrence, chiefly in Marsupials, Edentates, Rodents, and Ungulates, *i.e.*, in the lower orders of Mammals. 3. Opaque nerve fibres. All stages of opacity occur congenitally in Man, and are to be found normally throughout the Mammalia. Opaque nerve fibres are most marked in some of the Rodents and Marsupials. 4. Physiological cup and congenital discoloration of the disc frequently occur in Man. An appearance similar to the physiological cup occurs in all the Felidæ, and in a considerable number of the other Carnivora; also in the Flying Squirrels and some of the other Rodents. White and grey discs occur normally in a number of animals widely separated in classification, such as the Skunk, Rhinoceros, Porcupine, Armadillo, and Echidna. 5. Structures protruding from the disc into the corpus vitreum. A. Persistent hyaline artery. This congenital defect in Man is found as a normal condition in nearly all the Ruminants and in a large number of Rodents. B. Vestiges of a pecten. In some of the Rodents, more especially in all the Agoutis, a button-shaped vascular pigmented rudimentary pecten protrudes from the disc into the vitreous. It is remarkable to find in the Mammalia a relic of this Sauropsidan organ. In a number of Marsupials vascular protuberances from the disc into the vitreous occur in different forms. 6. Colobomata. The papillary

coloboma (Fuchs' Coloboma) has its analogy in a white or coloured scleral ring, which is normally met with in a large number of animals. 7. Retinitis pigmentosa. In the Galagos and Lorides a spreading of pigment occurs circumferentially in the retina, which greatly resembles Retinitis pigmentosa. If these nocturnal animals are exposed for prolonged periods to daylight the pigment advances concentrically, similar to the manner in which it progresses in Man, so that the animals gradually go blind. 8. Visible choroidal vessels and stippled fundus. Visible choroidal vessels occur in most of the Simiæ below Hylobates, and in a number of the other orders. They are most marked in the Macropodidæ, and some of the other Marsupials, which present the appearance observable in the extreme cases of the analogous congenital defect in Man. Stippled fundi are found in the feline Douroucouli and in the Lemurs, an appearance occasionally met with in Man. 9. Ectropion of the Uvea. In a number of the Ungulates, which have large oval pupils, pigmented excrescences of the iris are met with, and these evidently serve to screen the eye against glare, since their pupils only contract moderately to light. In the Hyracoidæ we meet with a distinct specialised organ, which can be projected from the iris towards the cornea, like a small screen, and this I propose to call the "Umbraculum."

The *divergence of the optic axes* follows the classification to a marked degree. The higher the order the nearer the axes approach parallel vision. Parallel vision with the power of convergence only occurs in those animals which possess a true macula, viz., Man and all the Simiæ. In other words, convergence appears to be the necessary outcome of a macula. This macula, which is bounded by a reflex ring, exists in all the Simiæ without exception, and in no other Mammals, so that it ceases with the last of the Simiæ.

If we eliminate the domestic animals in which the *refraction* varies over considerable limits in all directions, we find throughout the Mammalia, with a few notable exceptions, vision is hypermetropic. The eyes of amphibious and marine Mammals are adapted for vision in two ways. Those which live in fresh water have immensely developed ciliary muscles and proportionally increased accommodative power, enabling them to compensate for the loss of the refractive power of the cornea when the eye is submerged. In the marine Mammals, *i.e.*, Pinnipedia and Cetacea, not only is this ciliary muscle greatly developed, but there is always a large area of the cornea which is flattened in the horizontal meridian, producing an extraordinary degree of astigmatism.

Binocular Vision.—It seems that if Mammals below the Simiæ have binocular vision, they do not rely entirely on it. With the exception of Man and the Simiæ, Mammals very rarely move their eyes for the purposes of vision, but move their heads instead.

In all the Mammals below the Simiæ which have no macula we find a larger *sensitive area.* Sensitive areas of restricted dimensions, omitting those cases in which the area is limited to a macula, exist in the Carnivora, in which order the divergence is not great. In the Ungulates, Rodents, Edentates, and Marsupials, where we find great divergence of the axes, large corneæ, and nearly spherical lenses, the sensitive areas are larger, and probably the degree of difference in perception over such areas, compared with the more peripheral parts, is but little.

The great transparency of the retina and the extreme brilliancy of the reflecting surface of the choroid in the vast majority of Mammals, and an extraordinary prevalence of colours of every hue, lead one irresistibly to the conclusion that the rays of light do not form an image on the retina as usually taught, but that the image is formed behind the retina on the brilliant surface of the Tapetum or fusca pigment layer of the choroid, and is then reflected back on to the terminals of the bacillary layer. This arrangement for vision certainly bears a close resemblance to Lippmann's method of obtaining coloured negatives. He obtained negatives in natural colours by placing a reflecting mercury surface in direct contact with the sensitive film, thus reflecting the light which had traversed the film on to the particles of sensitised silver. In the eye the light passes through the nearly transparent retina (which is analogous to the photographic film) to be reflected from the Tapetum, or choroidal pigment, on to the terminals of the retinal elements (which may be compared to the particles of silver haloid). In Lippmann's device the colours are produced by interference. If we venture to carry our analogy still further, we may presume the same occurs in the eye. One difference between the two methods is that in nature the reflecting surface is always coloured, and only reflects a portion of the incident light. The colour of the fundus, however, is remarkable for the absence of blues and violets and the great prominence of red, yellow, and green colours. Yellow and orange are the prevailing colours in nocturnal animals. The peripheral area, which is characteristic of animals possessing a Tapetum, is usually dark brown, and reflects but feebly. It is probably nearly insensible to light, as it never occurs in animals having great divergence of the optic axes.

The eye is no exception to the rule that domestication greatly increases *variability.* The colour of the Fundus oculi of domesticated races differs not only from that of the wild species from which the races are supposed to be derived, but the colour varies also individually, an occurrence almost unknown in wild species. The influence of domestication is also indicated by the frequent occurrence of myopia and astigmatism. Myopia is almost unknown in wild animals, but it may occur in wild specimens which have long been kept in captivity.

Although no sound *classification* can be based on one single organ, a striking concordance exists between an attempted arrangement of the Mammalia according to the Fundus oculi and the most modern classification. The cases of disagreement are wonderfully few. These are restricted to the following:—

Chrysothrix leans towards the Arctopitheci. I find it necessary to separate the Galagos from the rest of the Lemurs—at least, as a subfamily. In the smaller Carnivores it is advisable to establish a separate family, the *Cynictidæ*, to include the otherwise viverrine genera, Cynictis and Galictis, together with Mephitis, hitherto placed with the Mustelidæ. The Sciuromorpha should be divided into Sciuridæ and Pteromyidæ, and Castor should decidedly be removed into the Hystricomorpha group, perhaps into the vicinity of the Octodontidæ. The Bats rank very low so far as the eye is concerned, possibly on account of their nocturnal habits. Among the Marsupials the Diprotodontia are decidedly lower than the Polyprotodontia chiefly on account of the high degree of development of the eyes of the Didelphidæ and Dasyuridæ. Since we meet with genera of the lowest type along with others of the highest type of retinal vascularisation, and again some without and others with the additional relic or specialisation of a Tapetum, it follows that the details of the vascularisation and of the Tapetum have been developed independently in the various main branches of the Mammalia.

In fine, the whole Fundus oculi affords a striking illustration of the working of progressive evolution, an example all the more valuable, since it illustrates the direct modifying effect of external factors upon a highly specialised organ—in the present case the continued influence of light upon the eye.

"The Influence of Increased Atmospheric Pressure on the Circulation of the Blood. (Preliminary Note.)" By LEONARD HILL, M.B. Communicated by Dr. MOTT, F.R.S. Received March 22,—Read May 17, 1900.

Paul Bert* recorded the arterial pressure in two dogs which he introduced, together with the kymograph, into a chamber, and submitted to a + pressure of 53 cm. Hg. The atmospheric pressure was raised to this height in the course of three-quarters of an hour. The mean arterial pressure rose in one dog 16 mm. Hg., in the other 16 mm. Hg.; the pulse frequency fell in the first from 216 to 200, and the respiration from 41 to 29 per minute. The respiratory oscillations of blood-pressure became increased. Bert ascribes the results

* 'Pression Barométrique,' Paris, 1878, p. 838.

as due to the diminution of the volume of the intestines—which results from the compression of the intestinal gas—and the consequent increased play of the intrathoracic organs. He was confirmed in this opinion by the fact that the substitution of oxygen for air made no difference in the results.

A. Smith* attributes the symptoms which arise in caisson workers partly to the effect of the increased pressure on the nervous tissue, and partly to the congestion of the blood in the neural axis. He supposes that the blood is driven by the compressed air from the periphery to the cranio-vertebral cavity. This mechanical explanation of caisson disease is contrary to the supposition which, theoretically, seems correct, viz., that the atmospheric pressure is equally transmitted by the blood to all parts.

I have put the matter to the test of experiment.

Method.—An anæsthetised dog or cat is placed at the bottom of an autoclave after the insertion of a cannula in the carotid artery, and, in some cases, of one in the vena cava superior, has been carried out. The cannulæ are connected with Hg. monometers, and these, together with a slow-movement recording drum, are also placed in the autoclave.

The drum is then started and the monometers set to record the arterial and venous pressures. The lid of the autoclave is next screwed on, an oxygen bottle connected with the inlet tube and the pressure in the autoclave rapidly raised to + two atmospheres (30 lbs.). The outlet tap is finally opened, the pressure rapidly lowered, the lid taken off, and the record observed. The times of increasing and diminishing atmospheric pressure are noted with a watch. The whole operation only takes two or three minutes.

Results.—Although exposed to this rapid change of atmospheric pressure, the circulation of the blood scarcely varies. The arterial pressure and venous pressures either continue at the same level or very slightly fall. The respiratory oscillations are increased, and the pulse becomes slightly less frequent. The gas injected contains, roughly, 80 per cent. oxygen, and thus the oxygen tension is raised from 21 per cent. to about 190 per cent. of an atmosphere. The results are the same whether compressed air or oxygen are employed.

Conclusion.—The mechanical congestion theory of caisson disease is untenable.

The expenses of this research have been met by a grant from the Royal Society Government Grant.

---

* Article "Caisson Disease," 'Allbutt's System of Medicine,' vol. 7, p. 38, 1899.

"On Cerebral Anæmia and the Effects which follow Ligation of the Cerebral Arteries." By LEONARD HILL, M.B. Communicated by Dr. MOTT, F.R.S. Received March 22,—Read May 17, 1900.

(Abstract.)

1. *Cerebral Anæmia produced by Immobilisation in the Erect Posture.*

Many hutch rabbits when immobilised in the erect posture become convulsed and after a short period of time die (10′—20′) from failure of respiration.*

The blood, owing to its weight, congests within the abdominal vessels, while the abdominal viscera drag on and so kink the vena cava inferior. The heart, therefore, gradually empties, and the cerebral circulation ceases. These results are due to the flaccid and atonic nature of the abdominal wall of the hutch rabbits. Chloralisation hastens the onset of death by dilating the arteries and by stopping the convulsions, for the spasms help to return the venous blood to the heart. Compression of the abdomen by a bandage, or immersion of the animal in a bath of water up to the neck, entirely prevents the onset of symptoms. In the case of the bath the hydrostatic pressure of the water outside balances, but not completely, the hydrostatic pressure of the blood within. At the same time the water causes the viscera to float upwards, and so removes the kinking of the vena cava inferior.

Wild rabbits, owing to the better tone of their abdominal muscles, are not affected by immobilisation in the erect posture until after the lapse of some hours, and the same is the case in respect of dogs, cats, and monkeys. When the tone of the skeletal and vascular muscle becomes exhausted in these animals, owing to exposure, shock, &c., or is abolished by anæsthetics, the blood congests to the lower parts. Death finally results from cerebral anæmia. Immersion in a bath or compression of the abdomen has the same restorative effect on the circulation of these animals as on that of the hutch rabbit.

Intense congestion and œdema of the lower parts, accompanied by thirst, is said to occur in men under like conditions, and death no doubt results from cerebral anæmia.

Hutch rabbits, when thrown into syncope by immobilisation in the erect posture, recover almost immediately on their return to the horizontal position, or on immersion in a bath, or on compression of the abdomen. The animals when returned to the horizontal posture may

* This fact was noted by Salathé.

be paralysed for a few minutes, and tumble and walk on the back of the fore-paws, but these symptoms quickly disappear even though the pupils' light reflex may have been abolished for 15′—20′.

2. *Cortical Excitability after Ligation of Cerebral Arteries.*

The author has produced contra-lateral clonic spasms in himself by sudden compression of one carotid artery. Consciousness of the cortical discharge arises from the sensations received from the parts in movement. The cortical discharge itself is unaccompanied with consciousness.

The cortex cerebri of dogs remains excitable and even hyper-excitable to electrical excitation after both carotid and vertebral arteries have been tied. The brain under these conditions is supplied with blood by the branches of the superior intercostal artery which enter the anterior spinal artery.

The brain is, however, rendered profoundly anæmic by the operation, and the animals are in consequence rendered more or less demented, anæsthetic, and paralysed. The paralysis results from a block established by the anæmia in the sensory projection, and association fields of the cortex cerebri for the motor cells are unaffected, in so far as not only purposive movements but typical fits can be excited on stimulating the "motor area." The "motor centres" are clearly not autonomous, and these experiments confirm the previous deductions concerning the origin of the paralysis which was obtained by Mott and Sherrington after division of the posterior nerve-roots of a limb, and by Exner after circumvallation of the cortex. Isolation of the cortical motor cells from sensory impulses produces paralysis, although the cells remain directly excitable.

In many monkeys the two carotids, and even the two carotid and one vertebral artery, can be tied without lessening the cortical excitability. The ligation of all four arteries is followed within one minute by loss of cortical excitability. In some monkeys (especially those in bad condition) ligation of both carotids abolishes the excitability.

3. *The Effect of Absinthe after Ligation of the Cerebral Arteries.*

The injection of absinthe, after ligation of the four cerebral arteries in cats and of the two carotid arteries in monkeys, produces as a rule only extensor rigidity of the fore limbs and dyspnœic respiration. No clonic convulsions occur. On loosening the carotids violent clonic and tonic convulsions ensue, and these can be again cut short by re-clamping the carotids. So soon as the carotids are re-clamped, the extensor rigidity reappears. It is deduced from these experiments that clonus is of cortical, and tonus of sub-cortical, and probably of cerebellar, origin.

4. *The Late Effects which follow Ligation of the Four Cerebral Arteries.*

These vary in different animals. Almost all rabbits die from failure of respiration within three minutes, after convulsions of an asphyxial type. There occur vagal inhibition of the heart, general vaso-constriction, and a high arterial pressure, prior to the failure of the circulatory system. The symptoms are in every way similar to those produced by clamping the trachea.

Cats become comatose and die within a few hours from respiratory paralysis, which is gradual in onset. Cheyne Stokes respiration sometimes results, and at a later stage long-drawn spasmodic gasps of the diaphragm occur at rare intervals. Extensor rigidity often occurs before death. Cats may survive the ligation of the two carotids and one vertebral artery.

Dogs recover from ligation of the four cerebral arteries, some after scarcely any symptoms, others after passing through a stage of dementia, accompanied by paralysis and anæsthesia, which lasts three or four days. The dogs during this period of dementia behave exactly like the dogs in which Goltz produced extensive destruction of the cerebrum. The spatial sensations depending on the nerves of skin, joints, and muscles are no longer brought into association with the sensations which are derived from the higher senses. Reflex defence and locomotor movements alone persist. Monkeys almost all die within twenty-four hours after ligation of the two carotids and one vertebral artery. The animals become soporose and then comatose. Extensor spasms, extensor rigidity, and failure of respiration follow. Monkeys recover without symptoms after ligation of both carotids.

In one monkey, after the two carotids and one vertebral artery had been tied, there ensued extensor rigidity and profound paralysis and dementia. This animal was kept alive by spoon feeding and continued in the same state. It was killed on the fifth day. The stage of sopor and coma or dementia may not appear in dogs or monkeys for an hour or so after the ligation of the arteries has been effected. On recovering from the anæsthetic the animals may at first appear lively and intelligent.

In man the ligation of one carotid artery is not free from risk, while the ligation of both carotids is recognised as a most dangerous operation. The two arteries can be tied successfully at intervals of time.

Attention is more particularly drawn to the following conclusions deduced from this research:—

1. The cerebral circulation of man, in the erect posture, depends on the tone and activity of the skeletal and respiratory muscles. The blood and lymph are returned from the lower parts to the heart by the expressive action of the muscles and constant change of posture.

2. The functions of the brain may continue after a great diminution

in blood supply. This substantiates previous work of the author in regard to the slight metabolism of the brain as measured by the exchange in blood gases. At the same time it does not favour the anæmic theory of sleep.

3. The electrical excitability of the motor area of the cortex cerebri persists when the sensory side of the brain is to a large extent paralysed, and the animals rendered more or less demented by profound cerebral anæmia.

4. The functions of the brain rapidly return so soon as efficient anastomosis is established. The period of partial paralysis and dementia lasts in dogs two or three days. Rabbits recover after the pupil reflex has been abolished for 15″—20″.

5. The limits between the degree of anæmia required to produce dementia and that which paralyses the respiratory centre are extremely narrow. For example, monkeys recover *without symptoms* after ligation of both carotids, but as a rule die after ligation of both carotids and one vertebral artery.

6. There is considerable variation in the number of arteries which can be safely tied in various animals, *e.g.*, in man, birds, goats, and horses (Mayer) one carotid; in monkeys both carotids; in rabbits and cats both carotids and often both carotids and one vertebral; in dogs both carotids and both vertebrals.

7. The four cerebral arteries can be safely tied in monkeys in successive operations.

8. The cortex cerebri is the place of discharge of clonic convulsions. Tonus is of sub-cortical origin. The clonic stage of an epileptic fit can be cut short by compression of both carotid arteries.

Dr. Mott, to whom I am greatly indebted for help and advice in this research, has determined by microscopical examination of the anæmic brains by Nissl's method, that—

1. The cortical cells in the brains of the demented animals are swollen and diffusely stained. The stichochrome granules are absent. The nuclei are swollen. The veins are congested and there may occur hæmorrhages in the cortex.

2. The large pyramidal cells are least affected.

3. The changes occur very rapidly after ligation of the cerebral arteries, and disappear synchronously with the recovery of the animals from the stage of dementia.

In the case of the monkey, described above, the cerebrum was softened in patches, many of the cortical cells were degenerated, and there were signs of active phagocytosis. No changes in the neurons were displayed by the Golgi method.

The expenses of this research have been met by grants from the Royal Society Government Grant.

"The Circulation of the Surface Waters of the North Atlantic Ocean." By H. N. DICKSON, B.Sc. Communicated by Sir JOHN MURRAY, K.C.B., F.R.S. Received March 23,—Read May 17, 1900.

(Abstract.)

In this paper an attempt is made to investigate the normal circulation of the surface waters of the Atlantic Ocean north of 40° N. lat., and its changes, by means of a series of synoptic charts showing the distribution of temperature and salinity over the area for each month of the two years 1896 and 1897.

The temperature observations discussed (numbering over 16,000) were obtained from the meteorological and hydrographical departments of the countries bordering on the North Atlantic, and special arrangements were made with the officers of a number of ships for the continuous supply of samples of surface waters for analysis.

The salinity of the samples obtained was determined by volumetric estimations of the amount of chlorine present. Over 4,000 samples were dealt with in this way, and special attention was devoted to ascertaining the accuracy of the methods employed. A large number of estimations were also made of sulphates present in the waters, and the limits of variation from a definite ratio of chloride salinity to sulphate salinity determined.

The specific gravity of over 500 of the samples was determined with the pyknometer, and a formula connecting the results of these determinations with the salinities derived from chlorines investigated.

The numerical results of the chemical and physical determinations are exhibited in a table, forming a substantial addition to the material available for the discussion of oceanographical problems of this kind.

The principal conclusions arrived at with reference to the circulation may be summed up as follows:—

1. The surface waters along the whole of the eastern seaboard of North America north of (about) lat. 30° N., consisting partly of water brought from the equatorial currents by the Gulf Stream, and partly of water brought down by the Labrador current, are drifted eastward across the Atlantic towards south-western Europe, and banked up against the land outside the continental shelf. This continues all the year round, but it is strongest in summer, when the Atlantic anticyclone attains its greatest size and intensity; and the proportion of Gulf Stream water is greatest at that season.

2. The drifts in the northern part of the Atlantic area are under the control of the cyclones crossing it. The circulation set up accordingly reaches its maximum intensity in winter, and almost dies out in summer. In winter the drifts tend to the south-eastward from the

mouth of Davis Strait, eastward in mid-Atlantic, and north-eastward in the eastern region. In spring and autumn the movement is more easterly over the whole distance, and a larger quantity of water from the Labrador stream is therefore carried eastward.

3. The water banked up in the manner described in (1) escapes partly downwards, partly southwards, and partly northwards. It occupies the whole of the eastern basin of the North Atlantic, and to the north it extends westward to Davis Strait, being confined below 300 fathoms depth by the ridges connecting Europe, the Faeroes, Iceland, and Greenland. Above that level it escapes northward by a strong current through the Faeroe-Shetland Channel and between Faeroe and Iceland, and by the two branches of the Irminger stream, one west of Iceland the other west of Greenland.

(As it seems desirable that this northerly current should have a distinctive name, it might be well to call it the European stream, and its branches the Norwegian, Irminger, and Greenland streams respectively.)

The strength and volume of the European stream is liable to considerable variation, according to the form and position of the Atlantic anti-cyclone, which causes the amount of banked up water, and the proportions escaping northward and southward, to vary. It is also modified by the strength and direction of the surface drifts in its course. It is, however, always strongest in summer.

4. The Norwegian stream is by far the largest branch of the European, and it traverses the Norwegian Sea and enters the Arctic Ocean. The warm water thus sent northward melts enormous quantities of ice, and the fresh water derived from the ice moves southward in autumn, chiefly in a wide surface current, between Iceland and Jan Mayen, which may entirely cover other parts of the Norwegian stream. Part of the surface water also comes southward through the Denmark Strait, but the amount is much smaller, probably chiefly because the melting of the ice is slower, and the channel is longer blocked.

The Greenland branch of the European current also causes melting of ice in Davis Strait, but the warm winds from the American continent and the water received from the land are probably more effective in increasing the volume of the Labrador current.

5. The water from the melted ice is spread over the surface of the North Atlantic during late autumn and winter by the increasing drift circulation, and it is gradually absorbed by mixing with the underlying water.

6. The circulation described is liable to extensive irregular variations, corresponding to variations in the atmospheric circulation.

"Palæolithic Man in Africa." By Sir JOHN EVANS, K.C.B., F.R.S.
Received May 15,—Read May 31, 1900.

In April, 1896, just four years ago, I ventured to call the attention of the Society* to some palæolithic implements found in Somaliland by Mr. H. W. Seton-Karr. In doing so, I pointed out the absolute identity in form of these implements with those from the valley of the Somme and numerous other pleistocene deposits in North-western Europe and elsewhere; and I cited others from the high land adjoining the valley of the Nile and from other places in Northern and Southern Africa. I was at the same time careful to point out that though there could be no doubt as to this identity in form, no fossil mammalian or other remains had been found with these African implements. I did not, however, hesitate in claiming them as palæolithic.

Since the publication of my short note, an extensive collection of stone implements formed in Egypt by Mr. H. W. Seton-Karr has been acquired by the Mayer Museum at Liverpool. I have not had an opportunity of examining the specimens, but a detailed account† of them, with numerous illustrations, has been published by the Director of the Liverpool Museums, Dr. H. O. Forbes. The majority of the implements are of Neolithic Age or even of more recent date, and with the account of these I need not here concern myself; but the author is at considerable pains to dispute my view that the instruments of palæolithic forms belong to the Palæolithic Period. As he says, Mr. Seton-Karr's statement that he sometimes found spear-heads "on the ground surrounded by a mass of flakes and chips as though the people had dropped their work and fled," is very suggestive and important. He adds, however, that "one such occurrence is almost sufficient in itself, I venture to think, to disprove the high antiquity claimed by Sir John Evans for these implements."

Were it certain that the so-called spear-heads were really of palæolithic form, and had the flakes and chips been fitted on to them so as to reconstitute the original blocks of flint, as has been done in the case of undoubted palæolithic specimens by Mr. Spurrell and Mr. Worthington Smith, the question would still remain to be discussed as to the condition of the localities in relation to subäerial denudation.

It is, however, hardly necessary to discuss these points, as some recent discoveries made in Algeria will, I venture to think, go a long way towards settling the question. I propose, therefore, very briefly to state their nature. About sixty miles to the south-west of the town

* 'Roy. Soc. Proc.,' vol. 60, p. 19.

† 'Bull. Liverp. Mus.,' II, Nos. 3 and 4 (Jan. 20, 1900); 'Nature,' April 19, 1900, p. 597.

of Oran, and about ten miles to the north of Tlemcen, on the plateau of Remchi, about a mile to the south of the River Isser, lies a small lake known as Lac Karâr. It occupies a depression in lacustrine limestone of comparatively recent geological date, superimposed on beds of Lower Miocene Age. The level of the water, which is some 15° centigrade warmer than that of the ordinary springs of the district, and appears to be derived from some deep-seated source, seems to be about 600 feet higher than that of the River Isser. The lake originally filled a much larger part of the depression than it now does, and from its old bed a considerable amount of material has of late years been extracted for the Service des Ponts et Chaussées. This material consists of sand and gravel rich in iron pyrites, in the midst of which lie, pell-mell, bones of animals and stone implements fashioned by the hand of man.

These have for some years been diligently collected by M. Louis Gentil, a geologist, and form the subject of a memoir that has just appeared in 'l'Anthropologie'* by my friend M. Marcellin Boule, of the Galerie de Paléontologie at the Jardin des Plantes, Paris. Some 200 specimens of implements have been submitted to him, of various sizes, and all or nearly all of well-known palæolithic forms, including several with a broad chisel-like end, of which examples have been found in the laterite of Madras and the gravels of Madrid. They are for the most part formed of an eocene quartzite, though some smaller specimens of the type known as that of "le Moustier" are formed of flint. The *facies* of these latter is not so distinctly palæolithic as that of the former, of which some, through the kindness of M. Marcellin Boule, are exhibited.

The most important part of the discovery is that which relates to the mammalian remains found with the implements. These are of elephant, rhinoceros, horse, hippopotamus, pig, ox, sheep, and certain cervidæ. I will not detain the Society with the details given in M. Boule's memoir, but I may call attention to the fact that the elephant is not the African elephant, but one more nearly related to the quaternary or even pliocene elephants of Europe, to which the designation Atlanticus has been given. Some teeth seem closely allied to those of *E. meridionalis* and even *E. armeniacus*. Having regard to the whole fauna, M. Boule arrives at the conclusion that it is identical with that of the fossiliferous deposits of Algeria, which from their topographical or stratigraphical characteristics have been assigned to the Quaternary or Pleistocene Period. He also cites other instances in Algeria, such as Ternifine and a station near Aboukir, in which palæolithic implements have been found associated with the remains of a similar pleistocene fauna.

Altogether, these recent discoveries in Northern Africa tend immensely

* Tome XI, 1900.

to strengthen my position with regard to the truly palæolithic character of the implements found in other parts of that vast continent, and I am tempted to bring for comparison some few specimens from South Africa. One of these, found by Mr. J. C. Rickard at the junction of the Riet and Modder twenty years ago, is almost indistinguishable from those of the Lac Karâr, as is also one from the valley of the Embabaan in Swaziland. But the most remarkable is an implement of typically palæolithic character found in 1873 under 9 feet of stratified beds at Process-fontein, Victoria West, by Mr. E. J. Dunn.* May the day be not long distant when researches for the implements of palæolithic man may again be carried on, and trenches be dug in South Africa for peaceful instead of warlike purposes.

"Influence of the Temperature of Liquid Hydrogen on Bacteria." By ALLAN MACFADYEN, M.D., and SYDNEY ROWLAND, M.A. Communicated by LORD LISTER, P.R.S. Received and read May 31, 1900.

In a previous communication we have shown that the temperature of liquid air has no appreciable effect upon the vitality of microorganisms, even when they were exposed to this temperature for one week (about $-190°$ C.).†

We have now been able to execute preliminary experiments projected in our last paper as to the effect of a temperature as low as that of liquid hydrogen on bacterial life. As the approximate temperature of the air may be taken as 300° absolute, and liquid air as 80° absolute, hydrogen as 21° absolute, the ratio of these temperatures roughly is respectively as 15 : 4 : 1. In other words, then, the temperature of liquid hydrogen is about one-quarter that of liquid air, just as that of liquid air is about one-quarter of that of the average mean temperature. In subjecting bacteria, therefore, to the temperature of liquid hydrogen, we place them under conditions which, in severity of temperature, are as far removed from those of liquid air as are those of liquid air from that of the average summer temperature. By the kindness of Professor Dewar, the specimens of bacteria were cooled in liquid hydrogen at the Royal Institution. The following organisms were employed: *Bac. acidi lactici*, *B. typhosus*, *B. diphtheriæ*, *Proteus vulgaris*, *B. anthracis*, *B. coli communis*, *Staphylococcus pyogenes aureus*, *Spirillum choleræ*, *B. phosphorescens*, *B. pyocyaneus*, a Sarcina, and a yeast.

The above organisms in broth culture were sealed in thin glass tubes

* See also a paper by M. E. T. Hamy in the 'Bulletin du Muséum d'Histoire-Naturelle,' 1899, No. 6, p. 270.

† 'Roy. Soc. Proc.,' February 1, 1900; ibid., April 5, 1900.

and introduced directly into liquid hydrogen contained in a vacuum jacketed vessel immersed in liquid air. Under these conditions they were exposed to a temperature of about $-252°$ C. (21° absolute) for ten hours. At the end of the experiment the tubes were opened, and the contents examined microscopically and by culture. The results were entirely negative as regards any alteration in appearance or in vigour of growth of the micro-organisms. It would appear, therefore, that an exposure of ten hours to a temperature of about $-252°$ C. has no appreciable effect on the vitality of micro-organisms.

We hope to extend these observations upon the influence of the temperature of liquid hydrogen on vital phenomena, and to make them the subject of a future communication, and to discuss their bearing upon problems of vitality.

"Vapour-density of Bromine at High Temperatures.—Supplementary Note." By E. P. PERMAN, D.Sc., and G. A. S. ATKINSON, B.Sc. Communicated by Professor RAMSAY, F.R.S. Received April 28,—Read May 31, 1900.

The authors regret that they had overlooked a monograph by C. Langer and V. Meyer, entitled "Pyrochemische Untersuchungen," containing an account of some experiments on the vapour-density of bromine. Their method was to pass a mixture of bromine vapour and nitrogen into a porcelain tube with capillary ends placed in a furnace in a horizontal position. The bromine and nitrogen were then displaced by a current of carbon dioxide, the bromine being absorbed by potassium iodide solution, and the carbon dioxide by potash solution, while the nitrogen was collected and measured. Temperature was ascertained by displacing the tube full of air in a similar way. By this method Langer and Meyer carried out experiments at "Zimmer-temperatur" 100°, 900°, and 1200°.

The only results comparable with the authors' are those at 900°. Their results at this temperature are 5·478, 5·414, 5·433, 5·382, 5·59, mean 5·459 (air = 1), or 78·88 (H = 1). They say that *these results indicate that the vapour-density of bromine, even when diluted with eleven times its volume of air, is still normal at* 900°.

The mean of our results at this temperature is 78·6, and the density read from the curve (p. 17, vol. 66) is 78·8. This close agreement shows that Langer and Meyer's results really indicate a small amount of dissociation at 900°. Diminution of pressure appears to have little effect at that temperature, as Langer and Meyer used bromine much diluted with nitrogen, while in our experiments there was no decrease of density on reducing the pressure from 767 mm. to 365 mm.

It may be noted that Langer and Meyer give the boiling point of

bromine as 63°, whereas we have found the boiling point of specially purified samples to be very close to 58·9°. (See Ramsay and Young, 'Trans. Chem. Soc.,' vol. 49, p. 454 *et seq.*)

"The Sensitiveness of Silver and of some other Metals to Light." By Major-General J. WATERHOUSE, I.S.C. (late Assistant Surveyor-General of India). Communicated by Sir W. ABNEY, K.C.B., F.R.S. Received April 25,—Read May 31, 1900.

During some recent investigations on the Daguerreotype process, the question presented itself as to which of the elements forming the sensitive surface of the plate—the silver or the halogens—the sensitiveness was due? Now, although the fact that nearly all compounds of silver, especially the haloids, are more or less sensitive to, and decomposed by, the action of light, has long been known, the sensitiveness of metallic silver itself to light, though observed in 1842, by Moser, has never been generally recognised either by chemists or by photographers.

Moser's Experiment.—Before describing my own experiments, it may be as well to give a description of Moser's experiment taken from the original paper in 'Poggendorff's Annalen,' vol. 56, 1842, p. 210.

"A perfectly new silver plate was thoroughly cleaned and polished. A black tablet with various excised characters was fixed above it, without touching it, and the whole placed in the sun for two hours or more and directed towards it. After the plate, which naturally did not show the least change, was cooled, it was held over mercury, heated as usual to about 60° R. (167° F.). To my great delight a distinct image of the screen was produced in which those parts where the sunlight (which during the course of the experiments was always weak and changeable) had acted, had attracted a quantity of mercury. This interesting experiment was repeated several times with the same result. Sometimes the plates after having been placed in the mercurial vapours were exposed to those of iodine and then placed in the sun, by which the images usually improved."

"If we compare this remarkable fact of the action of light upon surfaces of silver with the above-mentioned phenomena produced by contact, we can no longer doubt that light acts on *all* bodies, modifying them so that they behave differently in condensing the vapours of mercury. A similar experiment was made with copper during unfavourable weather. The copper was not well polished, and, consequently, the image produced by the mercurial vapours was faint, although clearly visible. By exposing the plate to the vapour of iodine the image became stronger, and this method was found useful in experiments with copper. A plate of clean mirror-glass was

exposed in the same way to light and the action was as plain as on the silver, if the glass were afterwards breathed upon, the image remaining visible for a long time afterwards. We may therefore assume that *light acts on all bodies, and its influence may be tested by all vapours that adhere to the substance or act chemically upon it.*"

Robert Hunt's Views.—Although Robert Hunt recorded these experiments in his 'Researches on Light,' he does not seem to have repeated them as regards the direct action of light upon metallic silver, but to have paid more attention to Moser's experiments on the images produced by contact or proximity of dissimilar substances, and his theory of invisible light, as well as the effects of heat. Hunt's own experiments were chiefly carried out on copper plates, and led him to attribute Moser's results to calorific or thermic radiations rather than to light. Knorr, Karsten, Grove, and others seem also to have investigated Moser's theories, but again without taking notice of the fact of images, either directly visible or developable, being produced on metallic silver by the direct action of light.

I have not been able to find a record of a visible action of light upon ordinary silver plate, though its occurrence should be well known to silversmiths.

Carey Lea's Observations.—Carey Lea found that the three forms of allotropic silver he obtained were all sensitive to light: A the red soluble, and B the dark brown or blue insoluble variety, becoming brown after some hours' exposure, while C, the golden coloured, became lighter by exposure.*

Electrolytically Deposited Silver.—In a series of electrolytic experiments made in Calcutta, in 1892, I found that a golden-yellow or light olive-coloured deposit of silver on the cathode plate (silver or platinum) of a decomposition cell formed with two pure silver plates, as anode and cathode, or with a silver anode and platinum cathode, in distilled water, through which a weak current was passed, was slightly sensitive to light and became lighter in colour by exposure. This seems to confirm Carey Lea's observation, if my golden-yellow silver deposit was analogous to his C-product. My deposit being made by electrolysis of pure silver in fairly pure distilled water, must have been nearly pure silver with no traces of foreign substances beyond those contained in the silver or the water, unless there was a small amount of occluded hydrogen, which other experiments of the same series, but with a stronger current, showed might not be impossible.

Photo-electrical Observations in Calcutta.—In another series of observations on the electrical action of light upon silver made in Calcutta about the same time, and published in the 'Journal of the Asiatic Society of Bengal,' Part II, No. 1, for 1893, I found that the action of

* 'Phil. Mag.,' Ser. 5, vol. 32, p. 337 (1891).

bright sunlight on a plate of almost pure silver, as indicated by a very sensitive Rosenthal galvanometer, was to make the exposed half positive to the unexposed, or as zinc to copper, which would seem to point to some slight oxidising action. The observation, however, was a difficult one, not readily repeated with certainty, and no very definite result was obtained. The currents observed did not appear to be due to unequal heating of the two halves of the plate, because the direct application of heat to the exposed side produced at once a clearly marked current in the opposite direction. This effect was always the same, and could be readily repeated.

In further observations of the same series, in which pairs of pure silver plates were partly immersed in distilled water or good tap water, one plate being exposed to light while the other was covered, as in Becquerel's electric actinometer, the current in nearly all cases, though small, was as above, the exposed plate being positive to the unexposed, as it generally was when the silver plates were placed in dilute sulphuric, nitric, phosphoric, or hydrochloric acids.

Confirmation of Moser's Observation.—Recent observations of the action of light upon silver, made in connection with the working of the Daguerreotype process, have fully confirmed Moser's observation quoted above, and shown that silver, when exposed under ordinary conditions, shows distinct sensitiveness to light, and that not only can an invisible developable image be obtained, as was done by Moser, but by prolonging the exposure printed-out impressions are produced which are clearly visible after exposure. The difficulty is to make sure of obtaining a surface of pure silver, free from the presence of condensed gases or other foreign matter which might affect the plate and give it a sensitive surface. The silvered glass plates used have generally been cleaned and polished with well-washed tripoli, and the metal plates in the same way, sometimes after being well cleaned with fine emery paper, and, in some cases, after making the metal red hot.

Various Silver Surfaces Sensitive to Light.—Repeated observations with silver surfaces of different kinds, pure silver plates and foil, silver leaf on varnished glass, Daguerreotype plates and silvered glass, have shown that by an exposure in bright sunshine of about half an hour to one or two hours, under an ordinary black and white negative, or a cut-out design, photographic images can be obtained, which are sometimes clearly visible after exposure, under favourable conditions.

Development of Images on Plain Silver Plates.—Whether visible or invisible, these images can be developed by the vapour of mercury, or by ordinary physical development with acid solutions of ferrous sulphate or pyrogallic acid, to which a little silver nitrate is added, as in the old wet collodion process. The images were also produced when the negatives or cut-out masks were separated from the silver surfaces

by sheets of thin mica, which, as Dr. W. J. Russell, F.R.S., has shown, stops the action of vapours upon a sensitive gelatine dry plate, and no doubt would do so upon a silver plate. As a rule, the mica itself makes no difference in the action of light, and is quite transparent to it, though, in some cases, it may exercise a slight retarding action. It was found, however, that when the silver surfaces were exposed to light in hydrocarbons, such as fluid paraffin, benzene, turpentine, &c., no action took place under the parts screened by mica, though the fluids were perfectly clear and colourless. Under ordinary conditions of exposure in a printing frame, the mica appeared to exercise no special influence upon the plates, except at the cut edges, as will be noticed further on.

Effects of Pressure.—That the images were not produced by differences of pressure was proved by leaving the mica screen, with the cut-out black paper design at its back, in contact with a polished silvered glass plate in darkness for twenty-four hours. There was no visible image, but on development with mercury vapour the mercury was deposited fairly evenly all over the plate, except just where the outside edges of the mica and the edges of some initials cut in it had been in contact with the silver. Here there was very little deposit, and the edges appeared as dark lines on a white ground. There was no sign of the black paper design. The fact that the images were readily obtained from ordinary black and white photographic negatives is further evidence against the results being due to pressure.

First Experiment with Silver Leaf.—My first experiment was made on the 14th August last with a piece of silver leaf laid down on a plate of glass coated with varnish, and exposed in the sun for a short time under a plain cut-out cardboard screen. On developing with mercury vapour a faint image was visible, the mercury having deposited on the part exposed to light.

Next day the experiment was repeated. A similar plate was exposed under a black and white gelatine negative of some lace for half an hour in bright sunshine. On developing with mercury, a very distinct, though faint, image of the lace pattern was produced, the mercury again depositing on the parts exposed to light, as in Moser's experiment.

Effect on Polished Silvered Glass.—Thinking that these results might be partly due to some action of light upon the varnish underlying the silver leaf, a piece of polished silvered glass was exposed under the same negative for about an hour. Again a distinct image was developed with the mercury, but partly positive and partly negative. A somewhat similar result was obtained on another plate developed with the ordinary acid iron and silver developer already noticed.

Effect on Pure Silver Plate.—The next experiment was with a piece of nearly pure silver plate, carefully cleaned with "Globe" polish, and

washed with benzene to remove all greasiness. After half an hour's exposure in dull sunlight and development with mercury, an image was produced similar to the two first on silver leaf, *i.e.*, the mercury had deposited upon the exposed parts.

These experiments with three different forms of silver surfaces and two methods of development, all giving results similar to those obtained by Moser, seemed at any rate to prove the correctness of his observation, and the sensitiveness of ordinary forms of silver to light.

With the exception of the first, the foregoing trials were all made with a black and white negative on glass. Others were then made with cut-out black paper screens and with like results.

Observation of Printed-out Visible Image.—The first observation of an image visible on the silver surface after exposure and without any development was on a plate of nearly pure silver, cleaned with tripoli and ammonia, polished off with dry tripoli, and exposed on the 21st August for about half an hour in sunshine under a black paper cut-out screen. The parts exposed to light appeared lighter than the unexposed, but on development the mercury was deposited upon the unexposed parts. In this case, the surface may have been affected by the ammonia used in cleaning, but it is also likely that the black paper used for the mask exercised some effect, as was afterwards found to be the case.

Another silver plate exposed for the same time under the same cut-out mask, but separated from it by a sheet of mica, did not show the visible image after exposure, though an image was readily developed with mercury vapour.

A day or two later, on the 24th August, the same experiment was repeated upon a plate of silvered glass exposed in the sun for half an hour under the cut-out mask, with a mica screen between it and the silvered surface. The image of the black paper design could be faintly but distinctly discerned in a suitable light, again appearing dark upon a lighter ground. Development with acid iron and silver brought out clearly the images of the paper mask, and of some letters cut out of the mica screen, as well as the edges of the mica screen itself.

A piece of silvered glass was then exposed for forty-five minutes under the same black and white negative as used in the first experiments, the silvered surface being protected from contact with the negative by a mica screen. In this case also a faint image was visible after exposure.

Several other prints, both from the lace negative and the paper masks, were made on silvered glass with longer exposures, so as to obtain a distinctly visible image, and it was then found that there was a tendency to reversal of the image when developed with mercury

vapour, *i.e.*, the mercury deposited upon the unexposed parts instead of upon the exposed. In one plate the image thus produced from the negative of lace, exposed for two hours in the bright August sunshine, has quite the appearance of an ordinary Daguerreotype picture on an iodised silver plate, though no halogen or other sensitiser was used.

Further trials of prolonged exposures of pure silver foil or plates carefully cleaned with dry tripoli powder have given very distinct printed-out images on the metal, so that there is no doubt about the fact that visible images can be produced on clean plain silver surfaces by light.

Blue Rays found to be Active.—In order to ascertain, if possible, what rays were active in producing these visible images upon the silver, and as it was hopeless to expect to obtain satisfactory results from observations with the solar spectrum, a slip of silvered glass was exposed under a small artificial spectrum of seven coloured glasses. After forty-five minutes' exposure in sunshine, very faint images of the violet and cobalt blue glasses were distinguishable, but showed more distinctly when breathed upon. A similar result was obtained upon a pure silver plate, and on developing with acid iron and silver the space exposed under the cobalt blue glass developed out quite clearly, with traces of the violet and blue-green glasses. This result quite agrees with an observation made by Moser that only the blue and violet rays have any influence on pure silver, for he obtained very clear images by means of glasses of these colours, while only traces could be rendered visible when red glasses were employed, although they transmit more light and heat.

On another silvered glass plate, exposed under a similar colour screen or artificial spectrum, consisting of fifteen coloured glasses, for three hours in bright sunshine, the image has apparently reversed by over-exposure, the mercury being deposited on the spaces exposed under the red, orange, yellow, and yellow-green glasses, but not on those which were under the blue-green, blue, and violet glasses.

Developable Images produced on Silver by Heat.—As we have seen, Robert Hunt was inclined to attribute Moser's results to the effect of heat or differences of relative temperature, rather than to light or solar radiations, but his experiments were mostly carried out on copper, which is, as I have found myself, much more sensitive to rays of low refrangibility and to heat than silver.

Towards the end of September, when the weather was much cooler than it had been at the commencement of my experiments in August, I found that there was a distinct falling off in the sensitiveness of the silver surfaces, and it seemed that Hunt's view might, at any rate to some extent, be correct. I therefore tried an experiment to see if the same developable images could be obtained by heat as by light. A silvered glass plate was polished and put into a printing frame with

the cut-out paper mask and mica screen, in which were cut-out initials, just as if it were going to be exposed in the sun, but it was gently warmed for about five minutes over a spirit lamp, and then developed with mercury. The cut-out initials and edges of the mica came out distinctly in dark lines, just as they did in the pressure experiment but there was also a clear image of the black paper mask, which developed lighter than the ground, by the deposition of mercury, or the opposite of the ordinary action of light.

This is a very interesting observation, but recent repetitions of it with silvered glass plates and clean silver foil have quite failed to give such a distinct image of the black paper, though traces of it have been visible, and the edges of the mica and of the cut-out initials were always clearly impressed. In one case, when the silver foil was well heated to redness on both sides before being placed in the printing frame and the subsequent heating, no image of the initials was obtained, and only part of one edge of the mica screen with a faint trace of one corner of the paper mask where there was extra pressure. From this it would seem that heat does not play any active part in the production of the images, though the higher temperature of the summer sunshine, as well as its greater actinic power, may accelerate their formation by light. This is to a certain extent proved by the fact that the most perfect printed-out image obtained on a pure silver plate was exposed for three days at the end of September, when the thermometer exposed in the sun at the same time did not rise above 64° F., so that there could have been no question of heat producing the effect, as there might have been under the hot, clear sunshine of August.

Protection from Air.—In most of the experiments the plates were protected by glass during exposure, so that the outer air had no direct access to them. When plates were exposed under mica screens and without the protecting glass, the outside unprotected surface became distinctly yellow and tarnished during long exposures.

Under Surface of Silvered Glass Plate not Sensitive.—In order, however, to ascertain the effect of cutting off all atmospheric action on the exposed side, a silvered glass plate was exposed from the back or glass side under a cut-out mask made of thin aluminium sheet, and exposed for four days in October, two days being sunny and two cloudy. There was no *visible* image on either side of the plate after exposure, but breathing showed an image on both sides. The plate was then developed with acid iron and silver, and showed the image not very distinctly, but as development was prolonged traces of it appeared at the back of the silver film quite clear of deposit, so that apparently the developer had worked through the protected parts of the film, while the exposed part had taken the deposit of silver. When the plate was dry there was, curiously enough, no trace of the image on either side of the plate.

This experiment was repeated in January, two silvered glass plates being exposed face to face for fifteen days, of which five or six were sunny and the rest fairly bright, with the object of also seeing whether an impression could be made through the upper plate on to the silvered surface of the under one. On developing with mercury, a fairly clear image of the cut-out design was obtained on the inner surface of the exposed plate, dark on a lighter ground, but neither on the outer exposed silver surface of the upper plate nor on the silvered surface of the lower plate was there any trace of an image. The experiment was repeated with a similar result. The only other case in which images have been obtained through the silvered film was on a plate partly fumed with hydrogen peroxide and developed with mercury, but much overdone. As far as they go, the experiments show that the visible image is not produced on the silver except more or less in contact with the air. Further trials in bright sunshiny weather are required to prove this.

Images formed on Both Sides of Exposed Glass Plate.—In connection with this action through glass, a curious result obtained on a plain glass plate may be mentioned. A piece of clean glass plate was exposed for a day and a half in October under the same aluminium screen, with another glass plate over it, so as to protect the surface from the air. It showed a clear breath image, but mercury vapour did not produce any image. Iron development, however, brought out part of the design very clearly, the silver depositing upon the unexposed parts and giving an image darker and clearer than the ground. The image of the design appeared by breathing on both sides of the exposed glass plate, so that the action of light went through both plates.

Effect produced on Varnished Silver Glass.—With the same object of ascertaining whether the images could be produced when the silver surface was protected from the air by varnish, a silvered glass plate was coated over one half with photographic negative varnish and then exposed in the usual way, under the cut-out paper mask and mica screen, in October. After exposure for two days the image of the black paper mask, the cut-out initials, and edges of the mica screen were not reproduced so clearly as usual, though they appeared readily enough on the unvarnished half when breathed upon. The distinct heightening of the effect under the varnished part is apparently due to some chemical combination, probably oxidation, or the formation of an organic silver compound sensitive to light. When the varnish is removed, the strong image remains, and there is a distinct change of colour in the exposed part of the silver surface, which takes a sort of yellowish olive-grey tint. Recent repetitions of the experiment with silvered glass and with a plate of nearly pure silver gave exactly the same results. It may be noted that with iodised silver plates the same varnish has been found to exercise a decidedly retarding effect on printed-out images.

Probable Cause of the Effects described.—As regards the cause of the peculiar effects described, and the nature of the visible and invisible but developable images produced by the action of light upon plain silver surfaces, it is very difficult to give any definite opinion. It would seem that in this, as in most photographic processes, the first action of light is principally molecular, but if the exposure be prolonged and takes place in the air under ordinary conditions, there is a certain chemical decomposition of the surface of the plate, and the impressed image becomes distinctly visible.

Moser.—Moser was of opinion that the action of light does not necessarily consist in the separation of two chemically combined bodies, even in the Daguerreotype, in which process he maintained there is no separation of iodine from silver under the action of light. (We now know that the iodine evolved is absorbed by the underlying silver.) He brought forward his experiments on pure surfaces of silver, where, he says, there can be no possibility of a chemical action, to show that the effects of the Daguerreotype could be produced in quite a different way.

Waidele.—Waidele,* although he does not refer to Moser's observation of the direct action of light upon silver, and concerns himself more with the contact actions attributed to invisible light, concludes that Moser's effects are not produced by any action of invisible light, but must be rather explained by the action of atmospheres on bodies and the absorption of gases. He points out that polishing powders, such as tripoli, charcoal, &c., ordinarily contain moisture and absorbed gases, for which they have a strong attraction, and if used in this state they give up these vapours to the surface polished. If, however, they are heated to a red heat to drive off all moisture and gases, they then act as absorbents, drawing out the moisture and gaseous impurities from the surface of the metal, and producing a far purer and more perfect polish. He also notes the effect of carbonic acid, hydrogen, and other vapours on iodised silver Daguerreotype plates.

So far as my experiments go, I do not think that the nature of the polishing materials has had much influence on the results obtained, though it is an interesting point which should be looked into.

Roscoe.—Roscoe† also attributes the Moser effects to absorbed gases, and says:—"Almost all of the singular phenomena first investigated by Moser, and ascribed by him to the action of latent light, may be more rationally explained by the authenticated facts of the absorption of gases by solid bodies." He also refers more particularly to the contact or "breath" images produced by laying coins, &c., upon polished plates of metal.

Action of Gases.—It is evident that if there be any chemical action

* 'Pogg. Ann.,' vol. 59, 1843.

† Watts' Dict. of Chem., II, 1872, p. 805.

on the silver under the influence of light, it must be produced by the agency of gases or vapours contained—

(i) In the silver itself.

(ii) In the layer of air or condensed gases in immediate contact with the plate.

(iii) In the surrounding atmosphere.

(iv) In the masks or coverings under which the exposures are made.

What the exact nature of the decomposition may be, and to which of these agencies it is attributable, is not easy to ascertain, nor does the appearance of the visible image give much clue to it. From the dull-grey colour of the exposed parts it seems, however, probable that oxygen plays an important part in the action; but whether the oxygen is drawn from the air or is disengaged from the metal itself, or whether both actions take place with interactions of other gases occluded in the metal itself or present in the atmosphere in contact with its surface, there is nothing so far to definitely show. It seems also probable that, as in many other photographic processes, the presence of watery vapour is necessary to bring about the decomposition. These points require further investigation at a time of the year when the light is bright and the effects can be observed under the most favourable conditions. From the fact that in most of my experiments the external air has been excluded by the outer glasses of the printing frames, we may, I think, conclude that the effects are due more to the gases or vapours occluded in or attached to the silver surface or to the screens, rather than to any outside atmospheric influences.

Oxygen found in Silver by Graham.—With regard to the presence of oxygen in silver, Graham found that silver heated and allowed to cool in oxygen could occlude 0·745 volume of oxygen, which was permanently fixed in the metal at all temperatures below an incipient red heat. It did not tarnish the bright metallic surface of the silver or produce any appearance suggestive of the oxidation of a metal. He further showed that silver in the form of sponge can occlude 8 volumes of oxygen without any visible tarnish. Silver appears to have a relation to oxygen similar to that exhibited by platinum, palladium, and iron to hydrogen. Silver can also occlude variable quantities of hydrogen, nitrogen, and carbonic acid.*

Dumas's Observations.—According to Dumas,† 1 kilo. of pure silver, prepared by fusion with borax and nitre, was heated in a vacuum to a temperature not exceeding a dull red, about 500° or 600° C. The evolution of gas continued for about six hours, and it was received over mercury. The gas given off was pure oxygen, amounting to 47 c.c. at 0°, and 760 mm. of pressure for 1 kilo. of silver, and

* 'Phil. Trans.,' 1866, p. 434.

† 'Comptes Rendus de l'Acad. France,' vol. 86, 1878, p. 68.

weighing 82 milligrammes. In two other experiments, in which silver was fused in a more oxygenated atmosphere, as much as 158 c.c. of oxygen, weighing 226 milligrammes, and 174 c.c., weighing 249 milligrammes, were obtained from 1 kilo. of pure silver in each case. He found also that silver which contains oxygen does not lose it all in a cold vacuum.

Mallet's Observations.—Professor J. W. Mallet, in some investigations on the Atomic Weight of Aluminium, notes these results obtained by Dumas, and states that in his own observations of the same kind, but using a lime support for the silver heated in a hard glass tube to a moderate redness, he obtained only 34·63 c.c. of oxygen from 1 kilo. of silver.*

These different observations show that silver, apparently pure, may contain a very considerable quantity of oxygen, and I believe it is a well-known fact, especially in the Mints, that oxygen is nearly always present in silver, with or without hydrogen, carbonic acid, and other gaseous impurities in much smaller quantities.

Whether occluded oxygen is the cause of the photographic effects produced on surfaces of apparently pure silver, might readily be proved by extracting all the oxygen from a silver plate by the above methods, and then exposing the resulting purified silver to light, a similar plate from which the oxygen had not been extracted being exposed at the same time. I have not the appliances at my disposal for making this experiment.

Effects of Heating Silver Plates to Redness.—With regard, however, to the effect of simply heating the silver plates to redness, the following experiments may be of interest, and seem to show that the heating, whether accompanied by subsequent "blanching" or "pickling" in dilute sulphuric acid or not, causes a distinct loss of sensitiveness, and in some cases seems to destroy it entirely unless the exposure is greatly prolonged.

A piece of thin, pure silver plate was first of all heated to a red heat over a spirit lamp, then plunged into dilute sulphuric acid, and, after being washed with distilled water and dried, was exposed for about two days and a half, partly in sunshine, at the end of September last, under a screen of mica carrying a cut-out design in tinfoil, which had also been passed through the flame of the lamp. No visible image was produced, nor did one appear by breathing on the plate. Development with acid ferrous sulphate and silver nitrate brought out traces of the opaque tinfoil mask and of parts cut out of the mica. The free ends of the plate, which were exposed to the air all the time, did not show any distinct attraction for the developer.

Another plate of pure silver, well cleaned with "meteoric" polish (No. 2) and exposed as usual under a mica screen with black paper

* 'Phil. Trans.,' 1880, p. 1020.

mask, only one day longer than the last plate, gave a very strong visible image requiring no development. There must, therefore, have been a considerable difference in the conditions of the surfaces of the two plates.

In the plate which was heated and then treated with dilute sulphuric acid, it may be assumed that any surface layer of condensed gases must have been destroyed and a surface of pure metal exposed—which, as one would expect, was not visibly sensitive to light. In the other case, in which the plate was probably oxidised (it had been lying by for several years) and was simply well cleaned with a dry polishing powder, the surface of the metal was left so sensitive to light as to give a strong visible image. This difference of action in two plates which were exposed to the same conditions of light, seems to show that the effects produced by light on surfaces of metallic silver are chemical rather than merely molecular in their nature.

A repetition of this observation gave similar results. Further, a plate of thin silver foil, well heated over the spirit-lamp and exposed under a mica screen, which was also heated, for two days, including some hours of sunshine, showed no image, even on development with mercury, although distinctly visible images had been obtained on the same piece of foil when cleaned in the ordinary way and exposed under the same or similar screens. Further experiment on this point is still necessary.

Effects on Silver Surfaces fumed with Acid and other Vapours.—A good many experiments have been made with silvered glass plates or silver foil, fumed with various vapours, which might possibly be present in small quantities during the exposure to light, among them hydrogen peroxide, nitric acid, ammonia, also sulphurous acid; these plates have all shown visible and developable images of a somewhat similar character to those formed on the plain metal.

Nitric Acid.—By fuming silvered glass plates with ordinary pure nitric acid (1·420) or, better, with the same acid diluted with an equal volume of water, very clear positive printed-out images have been obtained, even with the comparatively short exposure of one hour in the February sun. With a longer exposure the image was clearer, the exposed parts appearing lighter than the protected parts, as is also the case with the plain silver surfaces.

Dilute Ammonia.—Dilute ammonia containing one part of the solution at 0·880 in 30 of water, gave a similar image, but the film was not so sensitive as with the nitric acid. Further experiments have however, to be made.

Dilute Sulphurous Acid.—Dilute sulphurous acid, formed by acidifying a solution of sodium sulphite with sulphuric acid, gave a weak image of much the same character.

Hydrogen Peroxide Solution.—Silvered glass plates, fumed with

various strengths of hydrogen peroxide solution, have given fairly sensitive films, sometimes a little white in appearance, but as a rule the printed-out images produced on exposure to light are just the reverse of those produced on plain plates and those fumed, as noted above, *i.e.*, the parts exposed appear darker than the unexposed parts and do not attract mercury vapour, so that a positive picture is produced from an ordinary black and white negative. By long exposure this effect is sometimes reversed. The visible images produced on silvered glass plates fumed with the peroxide quickly fade away, though the details may still remain visible on breathing. The same effect of fading out has also been observed on plain silver plates, most markedly on the thin foils.

Rain or Boiled Distilled Water.—Silver plates exposed under fresh, clean rain water, or boiled distilled water, gave distinct images on development with mercury, but not very readily, and further observations are necessary.

Exposures in Fluid Hydrocarbons.—In order to ascertain whether the images would be produced on silver surfaces exposed in fluid hydrocarbons containing no oxygen, silvered glass and pure silver foil were exposed in fluid paraffin, benzene, xylene, and toluene.

Paraffin.—In clear, colourless, fluid paraffin very strong dark olive-yellow images were produced by light on silvered glass and silver foil on the parts exposed freely in the fluid or under the parts cut out of a black cardboard mask but under a mica screen, which covered part of the black mask, and of the plate itself, there was no darkening action whatever. The darkening appears to be due to sulphuration, but why it should not occur under the mica is not clear, unless the latter cuts off the rays which are active in producing it.

Benzene.—Silver foil well cleaned and exposed in ordinary rectified benzene under a cut-out black paper mask, and also half covered with mica, showed very clear, yellow images of the cut-out design on the part uncovered by the mica, apparently by decomposition of thiophene or other sulphur compound present in the benzene. This darkening required strong sunshine to produce it, and on prolonging the exposure in the sun the metal became quite bronzed in the uncovered exposed part, while the part protected by the mica also took a light tint. It may be noted that, as a rule, the backs of these slips of foil (which were exposed in test-tubes) were not protected by any covering, but showed no perceptible change of colour: the darkening of the exposed foil was therefore solely an effect of strong light.

Effect of Heating the Foil.—A piece of the same foil was exposed in the same way in the same benzene after being heated to redness. This did not show such a strong image, even by long exposure in the sun.

A piece of pure foil (assay foil) was exposed in a purer sample of

benzene, and also gave a distinct yellow image as soon as the sun acted upon it after some days' exposure, the light being dull till near the end of the period. Another piece of the same foil, heated to redness and exposed at the same time, only showed a very pale yellowing.

Xylene.—A similar very faint, but browner, change of colour was noticed on a piece of the same foil, not heated, exposed for several days in commercial pure xylene.

With toluene, similar results were obtained after long exposure.

Effects of Light upon other Metals.—A few observations have been made as to the action of light upon other metals, but with the exception of lead none of them have proved very sensitive, and further work is necessary in good weather.

Gold.—Images developable with mercury have been obtained on gold leaf by prolonged exposures, but scarcely any trace of a developable image could be obtained on some highly-polished well-gilt buttons exposed for several days in good sunshine in October.

Lead Foil.—On pure lead foil I have obtained a very distinct darkened image visible after exposure. Very distinct darkening was also produced on lead foil exposed in pure benzene.

Copper.—On copper distinctly visible images have been obtainable sometimes, but the metal is not so sensitive as silver to light, though I have readily obtained heat images on it developable by mercury. Pure copper foil exposed in sunshine in pure benzene showed a distinct darkening, but exposed in xylene it did not change colour.

Nickel, Platinum, Aluminium, Palladium.—Nickel, aluminium, and platinum appear to be quite insensitive to light, but with a small button of palladium, kindly lent to me by Mr. T. Bolas, which was exposed for some days under a black paper mask, there appeared a slight but fairly distinct trace of deposit of mercury on the exposed parts. The spot was too small to make sure of.

Trial with Röntgen Rays.—With the kind assistance of Mr. F. H. Glew I was able to make a few experiments with the Röntgen rays upon silvered glass and pure silver plates, exposed for several hours to the rays, but without any visible or developable effect.

The above is a summary of my observations in this direction so far as they have gone. I hoped to have made them more complete, but the dull winter weather has been very much against such work. I hope, however, to go on with it during the summer, but think it advisable to publish the results already obtained now, in order that others may be able to extend them at the same time. They show, I think, that most of the phenomena that occur by the exposure of ordinary photographic plates containing haloid compounds of silver can be observed upon a plain silver plate exposed to light in the air under ordinary conditions. It seems not impossible, therefore, that the key to the hitherto unsolved problem of the production and constitution of

the so-called "latent photographic image" may be found within these limits.

Note, added June 6.—Since the above was written, I have tried the effect of fixing agents, such as hyposulphite of soda and weak solutions of potassium cyanide and ammonia, upon the printed-out images on silvered glass. Neither of them destroys the image, but a very peculiar effect was noticed when a somewhat indistinct printed-out image on silvered glass was treated with potassium cyanide. The unexposed parts wrinkled up, but not the exposed parts; after drying, the plate recovered its polished surface, but the exposed parts, instead of appearing light upon a darker ground, appeared darker than the unexposed parts—the image generally being much clearer and more distinct than it was at first. This effect points to a very strong physical modification of the thin silver film, but requires further examination with fresh images upon silver plates.

INDEX TO VOL. LXVI.

---

 2 R

END OF THE SIXTY-SIXTH VOLUME.

Harrison and Sons, Printers in Ordinary to Her Majesty, St. Martin's Lane.

FURTHER REPORTS TO THE MALARIA COMMITTEE

OF

THE ROYAL SOCIETY.

BY

S. R. CHRISTOPHERS, M.B., AND J. W. W. STEPHENS, M.D.

CONTENTS.

LONDON:
HARRISON AND SONS, ST. MARTIN'S LANE,
PRINTERS IN ORDINARY TO HER MAJESTY.

1900.

FURTHER REPORTS TO THE MALARIA COMMITTEE OF THE ROYAL SOCIETY.

"The Native as the Prime Agent in the Malarial Infection of Europeans." A Report to the Malaria Committee of the Royal Society. By S. R. CHRISTOPHERS, M.B., and J. W. W. STEPHENS, M.D. Received June 8, 1900.

I. *Breeding Places in their relation to Native Dwellings.*

The Formation of Breeding Places.—The breeding places of anopheles are of a somewhat special nature. Thus large bodies of water, or even large pools, rarely contain larvæ; and generally they are not found in pools which are of long standing, in which there is much vegetable matter; nor in pure spring water. Pools which are thick with suspended matter frequently contain many larvæ, and they were frequently to be found in Accra in pools brackish to the taste. One pool in which larvæ were very abundant contained as much as 0·6 per cent. of salt.

An essential condition is shelter. Anopheles larvæ are surface feeders and have a special arrangement by which they feed with the head twisted, so that the dorsal surface is brought to lie ventrally. A still surface is therefore necessary for them to thrive. The food of anopheles larvæ in the majority of specimens examined by us in Accra consisted of a unicellular organism (protococcus ?). This formed the great bulk of the contents of the alimentary canal in larvæ taken from a number of pools of varying appearance. The pools in which anopheles larvæ appear to flourish best have a very faint opalescence, or even a green scum of this organism on the surface.

The occurrence of natural pools suitable to the growth of anopheles depends upon the character of the country. Thus in the hilly parts of Sierra Leone, such natural pools were very common; but in sandy districts they occur very rarely.

Wherever such pools have been found, they have, however, in nearly every case contained anopheles larvæ, quite independently of the proximity of human dwellings. In the country around Accra, however, we were unable to find a single natural breeding place.

Although a district may apparently be free from natural breeding places, yet around human dwellings breeding places are provided. In the building of native huts it is the usual practice to form the walls, or to make a platform, of dried mud. This is generally manipulated in a pit dug close at hand. After the building is completed the pit remains, and so around each native hut there are one or more of these pits. In very dry districts, such as that of Accra, with a rainfall of only 27 inches, pits of considerable size are also excavated for collecting and storing rain water, so that in and around native villages we may have as many as thirty or forty pits of varying depth and capacity. Most of these pits form breeding places of a suitable nature. Thus in Accra district, where natural breeding places are of extreme rarity, artificial ones exist in many hundreds.

The relation of Breeding Places to Ground Water and Lagoons.—As the dry season progresses the majority of the pits become dry and cease to be breeding places. The soil and subjacent rock (sand, gravel, and sandstone) are both extremely permeable to water. The drying of these pits depends, therefore, more on the general subsidence of ground water than upon actual evaporation of the water in the pit itself. The result is that breeding places exist only where the ground water is sufficiently near the surface to be reached by the deeper excavations (6 to 10 feet). In the beginning of April only those pits marked as blue spots on the accompanying map contained water. Towards the end of April a further reduction had taken place; so that in the whole district the number of pools available for anopheles did not exceed a dozen.

From the permeability of the soil and rock and the absence of rain, the ground water level was very uniform throughout the district and approximated indeed to the sea level. In wells in Accra water is reached at depths corresponding with the height of the ground above sea level. On the higher parts of the district (40 feet to 60 feet or more above sea level) in the dry season the ground water is not reached in the numerous pits existing everywhere, and in consequence large areas there are free from breeding places during the continuance of this season.

Around the borders of the lagoons, however, which are a feature of the coast, the conditions are different. Here the ground water is reached within a few feet, and even in the driest season is found in some of the numerous pits dug along the margin by the natives; and in such situations many breeding grounds accordingly occur. The number of pits is usually large in such situations, on account of the large number of villages and dwellings the sites of which have been determined by the possibility of obtaining water in the dry season.

The indirect effect of even salt lagoons and low-lying salt marshes is seen in the case of an arm of lagoon which passes in behind Accra.

The accompanying map shows that its margins are thickly populated, and abound with anopheles breeding grounds. Well on in the dry season the breeding places shown around this marsh are, with the exception of the deep wells in Accra discussed later, the only ones in the district.

Similarly, on the shores of a lagoon at Christianborg there are crowded native dwellings, and shallow pits and wells where anopheles breed plentifully.

We shall show later that anopheles easily fly considerable distances (a quarter of a mile or more), and that a considerable proportion of those from native quarters are infected. In the occurrence of native dwellings and the existence of these pits lies probably the explanation of the generally received belief that lagoons are unhealthy.

The Effect of Rain.—At the end of an exceptionally dry season pools of water had been so reduced in number as to be found only on the lagoon margin. Two heavy rainstorms then fell in close succession, and the result was a filling of pools throughout the entire district. Only a certain number of these newly-formed pools retained water for more than twenty-four to forty-eight hours, so that comparatively few permanent pools were formed. The limited extent to which permanent pools were formed (*i.e.*, those lasting four days or longer) is shown by the heavier circles on the map.

These were more numerous in the low-lying portions than on the higher lands. This is well illustrated in passing from village B (*vide* map) downwards, to the groups of houses D and E. In village B one pool only was in existence on the fifth day. After passing the 40-feet contour line, most of the deeper pits contained water. In groups D and E, 20 feet lower, not only the pits but even superficial hollows a few inches only in depth contained water. A fortnight later the majority of these pits contained anopheles larvæ.

The Nature of the Breeding Places in Accra Town.—The greater portion of Accra town is in the dry season free from surface water. There are, however, a certain number of deep wells, varying from 15 to 35 feet in depth, scattered through a considerable portion of Accra. Many of these are kept covered and constantly in use; others are not used, or only occasionally, and are not covered. A considerable number of these latter were found by us to contain anopheles larvæ in large numbers, and no doubt many more, to which we were unable to gain access, contained larvæ.

The depth of the water from the mouth of the well did not seem to influence the breeding of anopheles, as one of the deepest wells in the town (35 feet) contained larvæ.

On descending these wells one frequently disturbed mosquitoes which may possibly have been using the well as a resort during the day.

Besides the wells which contained water, there were many dry wells and deep pits, which with the higher level of ground water occasioned by two heavy showers of rain contained water, and were now the breeding places of anopheles. Other pits of lesser depth were also present in numbers capable, with a still higher ground water level, of becoming sources of anopheles. There is, however, one portion of Accra where neither wells nor pits exist, which after heavy rain contained no surface pools. This portion lies in the centre of Accra (see Section).

II. *Anopheles in Native Dwellings.*

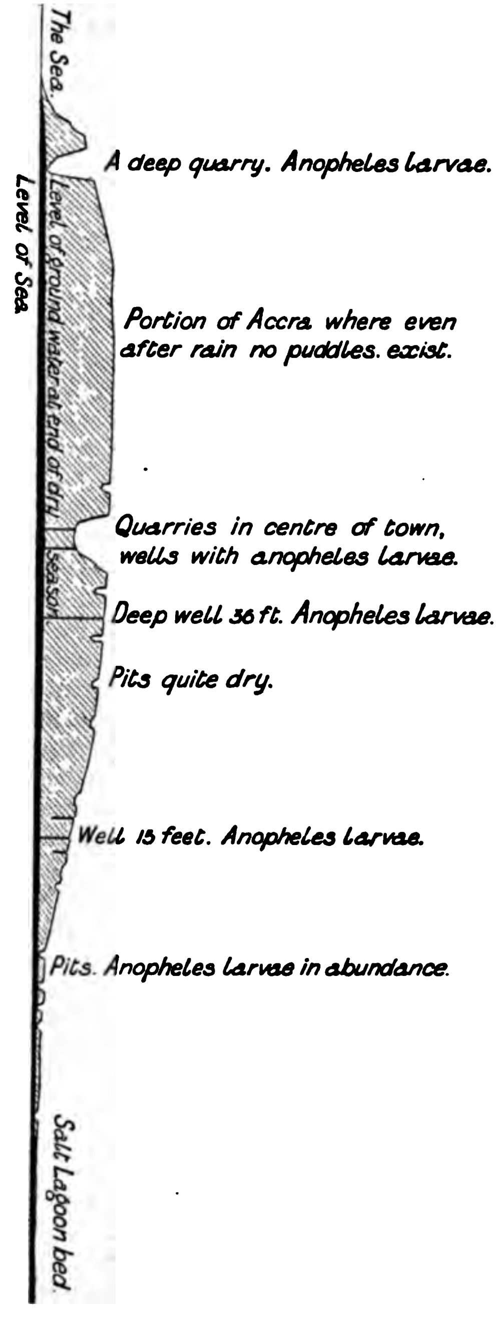
In the Presence of Local Breeding Places.—This condition we have already exemplified in Freetown, where in native dwellings, especially overcrowded ones, near the streams which teemed with larvæ, anopheles were caught in large numbers; whereas, on the contrary, in dwellings only a short distance (100—200 yards) away, they were very scanty. In Freetown we found that during the dry season the streams were the main sources of anopheles. In Accra the conditions are very different. Accra, with its scanty rainfall (27 inches) and sandy soil, may be considered the antithesis of Freetown. The native quarter of Jamestown is entirely dry, the soil is porous, and shallow pools do not exist in the town itself. We have still, however, a few deep wells in which anopheles larvæ were numerous, and on the outskirts larvæ were present even in April in a few shallow pools, which in the following week were dry. In Accra, though anopheles in the dry season of the year may be considered rare, or in some quarters possibly even absent, yet it may be noted that everywhere culex (several species) occurs in myriads, derived from tubs used by the natives for storing water. In these, culex larvæ were always present in astonishing quantity. A good example of a collection of native dwellings with breeding places occurs in a village close to Accra (village A, *vide* map); this village, extending along the margin of the salt lagoon, consists of some twenty huts, with numerous pigstyes and enclosures for domestic animals, all adjoining one another. The huts are of mud, mostly with grass roofs from which hang foul cobwebs and stalactites of soot. Among these, and among dirty clothing and bedding, anopheles hid themselves. It was therefore only with difficulty, even here, that under such circumstances anopheles were collected. Similar conditions were found in three isolated houses, where breeding places were in each case close at hand, and anopheles abundant.

In the Absence of Breeding Places.—Where breeding places have but recently disappeared, the number of anopheles does not undergo any perceptible diminution for some weeks. Thus, in one of the isolated houses just mentioned (house 1, *vide* map), situated on the dry lagoon

xplanation:-

- ▭ - Human infection.
- ● - Anopheles breeding grounds near end of dry season.
- o - Pits remaining filled for considerable time after heavy showers and capable of becoming sources of Anopheles.

its marked so o do not retain water for any length of time after heavy showers.



Section through Accra (Jamestown) from north to south.

bed, the sole local breeding place became dry on April 14th, the nearest breeding place being then over half a mile away. Nevertheless, on April 30th, in spite of several collections having been made between whiles from this house, anopheles were still numerous.

After longer periods of time, as shown both in Freetown and Accra, they eventually become rarer, and may even be apparently absent.

The detection of anopheles is not easy, when we consider the actual conditions under which the search is made. It is generally only possible to search a hut when the inmates have arisen from sleep, when the anopheles consequently have been disturbed, and when to avoid daylight they seek all the crevices and hiding places possible. If the room possesses a mud wall, they still may be found resting here gorged with blood; but where the walls and roof alike are formed of grass or palm leaves search, if delayed for any time, is frequently fruitless. Even then, however, they may still be found clinging to the thatch or rafters, or as mere formless lines on the sooty cobwebs which hang above the fireplace. The dawn of day is the most favourable time, and in European residences, where light penetrates everywhere, search later may be quite negative; indeed if anopheles are not found clinging to the outside of the mosquito net in the early morning, it may be safely concluded that they are absent. Once disturbed in this position they immediately fly off, and are not again seen until the next morning. The numbers detected by search also vary much, and a positive search may be followed by a negative, and *vice versâ*. In huts where little or no light penetrates, anopheles are content to remain; they are, however, as already stated, difficult to find here. We have, however, often detected them here while in European houses, a few yards off, they were very rare after daybreak.

The impossibility then of detecting anopheles in many houses where we thought they probably existed, led us to use more delicate means of determining their presence. We have described how in Freetown by constructing artificial pools of cement, and keeping them constantly supplied with water, larvæ appeared in them after some days; so that the freedom of many houses and areas was only apparent.

In the village B (*vide* map), we have an area which had been free from breeding places for some months. Here by cement pools anopheles were detected, whereas experience had taught us that search in such a district might have been fruitless.

Our experiments in Freetown led us to the conclusion that ova did not exist alive in the earth of dried pools. These experiments repeated here have given the same result, but we found that, provided that there were moisture still in the cracks of the mud, ova taken from these would survive. At the end of the dry season, however, there can be no doubt that living ova do not exist in the site of old pools. When rain first falls, then, we should have a series

of pools ready made over an extensive area, which may be used to detect the presence of anopheles where breeding places had been for long periods absent. However, before recording the distribution of anopheles at the end of the dry season, as shown by the first rains, we should be careful that the pool used as a test is a suitable one.

We find, then, that, as a result of the first rain shower, anopheles larvæ are found near the majority of native dwellings or even isolated huts, however free they may have appeared to be from anopheles. Apart from dwellings no widespread diffusion of anopheles occurred in the Accra district, as the result of the first rains. With the object therefore of detecting anopheles, where previously they might have been supposed to have been absent, pools were examined in various native quarters.

Village A.—Here before the rains, anopheles pools were in existence. They had, however, been reduced to one or two situated at the northern extremity of the village, a quarter of a mile away from the huts of the extreme southern portion. Four days after the rains, larvæ were present again in the pools of the southern portion.

Village B.—Anopheles had previously been detected here by means of a cement pool. None of the pools in this village held water sufficiently long for us to detect larvæ.

Village C.—In this village, although there was no water nearer than a quarter of a mile, and no anopheles had been detected breeding nearer than half a mile, yet in five days after the first tornado, larvæ occurred in the village pools. Search in the huts in this village had previously detected one adult anopheles only.

The Houssa Cantonment.—A still more striking instance of this condition, of which we have now found so many examples—viz., the existence of anopheles independently of local breeding grounds—was furnished by the military cantonment at Accra. This is situated about three miles north-east of Accra, and consists of a collection of Houssa huts and officers' quarters. Before the onset of the rains no surface water existed, either locally or in the surrounding country for a mile or more around. The water supply of the garrison was derived from a deep well about half a mile away, closed in, and consisting of pure clean water. From the porous nature of the soil, small collections of water formed in the drawing of water disappeared at once. We have then in this cantonment a native quarter in the dry season for many months devoid of breeding places, and yet here again, as before stated, we found by search three anopheles, an indication that more existed.

Although after the first rain an extensive shallow sheet of water existed immediately adjacent to the huts, yet the character of the water was not then suitable, and neither culex nor anopheles larvæ were detected in it.

An Isolated Hut.—A small hut, which appeared to be used merely as a temporary shelter on a cassava farm, was found about one mile distant from any other dwellings. Anopheles larvæ occurred here about ten days after the first rain, in a pit previously quite dry. The larvæ in this isolated spot may have been laid by mosquitoes lurking in the hut, or they may have been due to anopheles occurring in the bush, independent of man, as in Sierra Leone. The pit, however, was a well, and they were not found in pools apart from houses elsewhere at this time. These pools, however, were never suitable, so that it is difficult to say how far in Accra anopheles occur, as in Sierra Leone, apart from man.

Accra Town.—It has been previously mentioned that a large portion of Accra was free from any breeding places other than certain deep wells, which nevertheless are few in number and widely separated. There are, however, pits of various depths which in the rains become breeding places of anopheles, and these are both numerous and widespread.

Seven days after the first rainstorm, some of these still contained water. An area was therefore chosen as remote as possible from any breeding place previously existing, and several of these pits were examined. The result was that in the majority anopheles larvæ were detected. In this portion of Accra, then, where breeding places were encountered only at considerable distances, anopheles, as shown by the pool test, were at that time widely distributed. It is in this portion of the town that most of the commercial European residences are situated.

There is, however, a portion of Accra town where a different set of conditions exists. In the very midst of the town is an area, shown on the map by a dot-and-dash line, within which neither wells nor pits exist. After an extensive fire, which burnt down this quarter, well-formed roads with cement gutters were made, and though the dwellings here are very squalid, and many culex are breeding in the domestic pots, yet even after two heavy falls of rain no surface pools existed. It will be seen in the next section that this portion of Accra offers a striking contrast, in one point, to the whole of the rest of Accra.

As a result of these observations, we come to the conclusion that anopheles exist in the neighbourhood of native dwellings in great numbers where breeding grounds are present, and that they generally exist even where for long periods, sometimes for several months, there have been no breeding grounds.

The Flight of Anopheles.—How far this widespread occurrence of anopheles is due to flight from long distances we cannot definitely say. It is certain, however, that under some circumstances anopheles may fly much greater distances than has been supposed. Thus in a

small isolated house which had been occupied by one old man, five or six anopheles were to be caught each morning. These were both male and female, the latter of which usually possessed undeveloped ovaries and imperfectly matured salivary glands, such as only freshly hatched specimens show. These anopheles could be only derived from a breeding place in a village A at the foot of the hill. This would give 300—400 yards as a flight frequently undertaken.

Also along the narrow wind-swept ridge between the salt lagoon at Christianborg and the sea, numerous wells have been excavated to a depth of 10 to 12 feet. Many of these contain numerous anopheles larvæ, which occur also in the wells farthest removed from Christianborg. These are about 600 yards distant from any dwelling.

If the anopheles breeding here are not subsisting apart from man, they must fly backwards and forwards over a distance approaching half a mile. There are many reasons, however, for regarding such a distance as unusual. Thus, in our own bungalow at Accra, we did not find anopheles, although native quarters and breeding places existed under half a mile distant.

Whether anopheles can fly such distances or not, we do not think in the majority of cases the supply of a dwelling is kept up by such means, as one knows, in the case of isolated house 1 (map), large numbers of individuals may remain over for weeks. Also it is difficult to believe that in Accra the limited number breeding in the dry season travel far to take their food.

There seems a connection also between the number present in the dry season, and the number of breeding places which have existed in that region in the rains. This is apparently shown in Accra, where the limited area from which potential breeding places are absent shows a well marked reduction in the number of children infected, whilst a similarly dry area, but with potential breeding places, showed no such reduction.

In the rains new factors are present, and a spread of anopheles undoubtedly takes place.

III. *Malarial Infection in Native Dwellings.*

The Extent of Infection among Natives.—We have seen that native dwellings may show one of several conditions:

1. They may have breeding places close at hand, and be infested with large numbers of anopheles. The example given of this was village A (map), and the isolated houses on the marsh, 1, 2, 3.

In village A we examined, as far as possible, the blood of every child under 12. Certain of the huts did not contain children, and in a few cases we were unable to obtain specimens. The houses numbered 1 to 17 are shown on the map, those in which infected children were found being indicated by diagonal lines. No house with children that

was examined was without infection, the uncoloured houses being in every case either those without children or those not examined. Table I shows the number of cases which were found by a single examination of each child's blood.

Table I.—Table to show the Extent of Infection in the Children of a Village where Local Breeding Places are present. Village A.

House.		Ring forms.	Pigmented leucocytes.	Crescents.	Total infections.	Total number examined.
1	..	..	2	1	2	2
2	..	1	1	..	2	4
3	Not examined.					
4	..	1	..	..	1	4
5	..	..	..	1	1	2
6	Not examined.					
7	..	1	3	..	4	7
8	Not examined.					
9	..	..	1	1	2	4
10	..	..	1	..	..	1
11	Not examined.					
12	..	..	..	3	3	4
13	..	2	1	1	3	3
14	No children.					
15	No children.					
16	..	2	..	1	3	4
17	..	..	1	..	1	1
1	Isolated house.. ..	1	..	..	1	2
2	No children.					
3	Isolated house.. ..	..	..	2	2	3
	Total	18		10	25	41

This condition of general infection was also present in the isolated houses on the marsh, viz., in houses 2 and 3, the only ones containing children. It will be seen that two or more infected children may be present in each hut, while in house 7 four out of seven were infected.

Out of a total of 40 children 25 were infected. Of these 10 had crescents (gametocytes), whilst 18 had either ring forms or crowded pigment cells, indicating a present or recent attack. Seeing that one blood examination only was made of each child, it is certain that the children were suffering from an almost general infection, and were the source from which the infected anopheles, found in every house, derived their infection.

2. We next determined to what extent the infection differed from

the above in native quarters where local breeding places of anopheles were non-existent, where for considerable distances no water was available for breeding, but where nevertheless, as we have shown, a certain number of anopheles are generally present.

Village B, though containing numerous pits, was quite devoid of water in the dry season, and distant 200 yards from the nearest anopheles pool, which was situated in a group of houses at the foot of the hill. We were only able here to obtain a single blood film, but this showed numerous pigmented leucocytes.

Village C.—This was a small isolated village, situated on high sandy soil, with no water nearer than 400 yards, and no recognisable anopheles source nearer than half a mile. An examination of the children showed, however, an amount of infection not appreciably less than that shown in village A, where anopheles were breeding.

Out of ten houses examined six contained infected children. 15 children were examined in all, of these 8 showed crescents, whilst 5 gave evidence of recent infection with ring forms. (Table II.)

Table II.—Table to show the Extent of Infection in the Children of a Village where no Breeding Places are present. Village C.

House.		Ring forms.	Pigmented leucocytes.	Crescents.	Total infected children.	Total children examined.
1	..	..	1	2	2	4
2	No children.					
3	..	..	..	1	1	1
4	..	1	2	3	4	5
5	..	..	1	..	1	1
6	..	..	..	2	2	3
7	One child only ..	..	..	..	..	1
8	..	..	1	..	1	1
9	No children.					
10	No children.					
Total		6		8	11	16

The Military Cantonment.—In the cantonment, as we have previously shown, there exists an example of total absence of possible breeding places over a large circumferential area, and the condition has lasted at least as long as three months in the present year. Under such conditions one might reasonably have supposed that the amount of infection would be slight. On the contrary, we found here an extra-

ordinary degree of infection among the children. Out of 25 children examined at random 17 were infected, of which 6 showed crescents and 15 recent infections with ring forms. What conditions were responsible for such a high degree of infection we are unable to say. Anopheles, though probably present in greater numbers than they appeared to be, yet could not have been abundant. Three species of culex, very numerous in the huts, were examined, but yielded negative results. Inquiries showed that the men and their families had not been away from the cantonment for many months. At least it appears evident from these examples that the amount of infection in native children does not necessarily bear any very definite relation to the actual numbers of anopheles present. (*Vide* Table III.)

The Town of Accra.—In those portions of Accra where occasional breeding places occur, and where a considerable number exist during the rains, we have found an amount of infection not very different from those already mentioned. (4, 5, and 6, Table III.)

The central portion of Accra, to which we have already drawn attention as being without breeding places, even after rain, showed a very marked and striking difference. This was the first area in which the infected children were less than 50 per cent. In the position (8) this was especially marked, 5 out of 24 children only being infected; a number which is really less than it appears, as here for the most part very young children were selected.

Table III.—Table to show the Relation between Anopheles and the Amount of Infection in Native Children.

	Breeding places.	Anopheles.	Ring forms.	Pigmented leucocytes.	Crescents.	Total infections.	Total number examined.	Percentage of total infections.
Village A	Present	Considerable numbers	8	10	10	25	41	61
" B..........	Absent	Scanty	1	5	8	11	15	73
Cantonment	"	"	6	13	6	17	25	68
Accra, 4, 5, and 6 ..	Scanty	"	4	9	4	14	20	70
" 7 and 8.....	Absent	Extremely scanty or absent	0	4	7	10	32	31

The Nature of Infection in Natives.—Although among the children so large an amount of infection was present, yet in the adults parasites

were rarely found. Not only was this the case, but among the children themselves a diminution of infection occurred as the age increased. Young babies were infected in general in greatest proportion, then small children, whilst children of 10 or 12 were less commonly infected. Over 12, infected children were rare. Table IV shows this

Table IV.—Table to show Increasing Immunity with Age of Native Children.

	Babies.	Children up to 8.	Children up to 12.	Children over 12.
	Per cent.	Per cent.	Per cent.	
Village A.....	90	57	28	
„ B.....	75	50	..	very rarely infected.
Cantonment...	71	75	30	
Accra (7 and 8)	23	20	..	

diminished susceptibility as age increases, though the number of bloods examined is not sufficiently great to make these figures more than approximate. Out of 78 infections no parasite other than the malignant tertian was present, and though some infections were diagnosed from the occurrence of crowded pigmented leucocytes, yet these had the appearance of those encountered in malignant tertian cases.

In all but a few cases the infection was scanty, and apparently did not much inconvenience the child, as none of our specimens was taken from supposed cases of malaria, but from children running about and seemingly healthy.

Of the crescent cases only one or two showed them in any number. There was, however, a very rapid change of these bodies into round and flagellating bodies, and even in rapidly prepared dry films they had generally assumed this form. Intra-corpuscular developmental forms of crescents were also frequently encountered. The most curious condition of infection was that in which a few ring forms were encountered; these were frequently large forms, one-third or more the diameter of the corpuscle. In a few cases considerable "ring infections" were present, and in many others the number of pigmented leucocytes led one to think that a somewhat severe infection had lately taken place. It is unlikely that such a condition should be kept up for months without frequent reinfection, and the comparative immunity of the children in the central portions of Accra is against it.

The immunity of native races may possibly be an acquired one, from repeated infection during childhood; at any rate, there can be no lack of infection in native dwellings.

Infection of Anopheles in Native Dwellings.—Among anopheles caught in native quarters, both in Sierra Leone and Accra, we constantly find a certain proportion infected. Whether caught indiscriminately in towns or villages, or collected from individual houses, the result is the same; each batch, with rare exceptions, contains from 5 to 10 per cent. of infected specimens, a proportion which holds good at any rate for the dry season. What the proportion is during the rains we do not yet know.

In village A (map), in every house in which anopheles were caught they were infected. In the houses marked 1, 2, 3, and situated on the flat, grassy lagoon bed, we had houses exceptionally isolated, yet among anopheles caught here specimens were infected. The presence of infected anopheles in native quarters then appears to be not an occasional, but a constant, phenomenon, dependent undoubtedly upon the human infection in such places.

The Analogy between the Transmission of Malaria to Europeans and that of Ngana.—Bruce has shown that ngana is transmitted to domestic animals by means of the tsetse fly from the buffalo and wild game of a district. The trypanosoma was also found by him.

It has also been long noticed that with destruction of buffalo and wild game in a district, both the tsetse fly and ngana became less frequent.

In the very remarkable general infection of native children with an apparently mild form of malaria, and the close relation of anopheles to native dwellings shown by our investigations, and in the transmission thereby of malaria to Europeans, there is a process resembling that which takes place in ngana. Further, the analogy holds good in the conversion of the mild or innocuous type of the native disease into the virulent form in Europeans. We do, however, find severe forms also in natives. It may at least be pointed out that were a European in the Accra district (and no doubt the condition is a general one) to sleep in any native hut without a properly arranged mosquito net, he would be exposed to certain infection. Even were he to sleep a single night within a hundred yards of any native village, the risk would be very great. One of us derived his primary infection in this way six months after entering a highly malarious region. For the first time, whilst travelling, a night was spent in a native village, and, through an accident, without a net. Here, though present, anopheles were not very troublesome; nevertheless a severe infection resulted nine days later. Two other attacks were also traceable to exposure to bites from anopheles near native villages. Apart from such conditions we have never contracted malaria during a residence of fifteen months in highly malarial districts. We have also on several occasions slept, adequately protected, night after night, at the edge of swamps without any ill effect.

In most regions little frequented by Europeans, it is usual in travelling to utilise each night a hut in a native village. The attack of "fever," which is so usual after such journeys, becomes at once explicable on a knowledge of the conditions in native quarters. The gross carelessness with which mosquito nets are used—and only too frequently they are dispensed with altogether—contributes naturally to this end. (See also pp. 20—22.)

Also we have seen that towns on the West Coast of Africa differ but little in the amount of infection present from the villages, and here the European house is most frequently surrounded on every side by native hovels. In towns then, also, it is highly probable that a large number, if not the majority, of cases of malarial infection are derived, not from pre-existing cases in Europeans, but from native sources.

IV. *On the Segregation of Europeans.*

If we consider now the conditions here depicted, we see that from its peculiarities of scanty rainfall and dry soil, from the fact that in some of the areas described we have breeding places, in others not, that we have isolated huts and isolated villages with and without breeding places, and in the cantonment an isolated community under special conditions already described, we find that Accra presents us with a means of estimating the effect of various factors to which importance may be assigned in the spread of malarial infection.

We have seen that although a hut be isolated and without breeding places in the dry season, yet its inmates are infected, and infected anopheles likewise occur.

The same holds good for isolated villages, the infection presenting no appreciable difference from that of a village where there were still breeding places in the dry season.

The same condition held good for the isolated cantonment, also without breeding places in the dry season. Here, for some reason, the infection was particularly great, and the European officers were undoubtedly exposed to infection.

The same holds good for the greater part of Accra town with the exception already noted.

The conclusion from these data—and we have in Freetown partially expressed this conclusion—is that native quarters wherever existent will most certainly be sources of infection.

In a portion of Accra town, already indicated, a partial exception exists. Here probably there are no breeding places for any long period, even at the height of the rains. Although it has been shown to possess a striking diminution in infected children, yet the protection is not absolute, no doubt because it is surrounded by an infected population.

Hence the first means of obvious protection for Europeans consists in avoiding native quarters with their infected population and infected anopheles.

In Accra we have three districts of Jamestown (Accra town), Victoriaborg, and Christianborg. The first we have already described incidentally; it is composed mainly of the large native quarter, but in portions of this, or closely adjoining it, we find many of the commercial residences. A European dwelling in this quarter is thus living in the midst of dangerous sources of infection, and as it is only too common to find nets improperly used, the residents here are certain sooner or later to suffer from malaria.

In Christianborg, at the east end of Accra, the conditions do not differ materially from those in Jamestown, *i.e.*, we have here in close proximity to the European residences numerous native quarters; so that residence here constitutes exposure to serious risk of infection.

In Victoriaborg we have a different condition. This portion consists of official bungalows which occupy an isolated area, remote from both Accra town and Christianborg, and surrounded by a dry sandy area which even in the rains cannot afford many breeding grounds. It is only on the western side that the hospital and a few adjacent bungalows approach Jamestown. This condition of isolation constitutes an admirable feature, and the risk of primary infection here must necessarily be small. In the location of sites it is of the utmost importance that they should not approach the native quarters, and although bungalow 15 occupied by us was less than half a mile from the nearest large village north-west of Jamestown, yet throughout the dry season we did not detect a single anopheles.

Unfortunately no data exist to show where primary infections occur, nor do we find here any microscopical evidence of the malarial nature of febrile attacks, in some of which, indeed, we have been able by microscopical examination to exclude malaria. We must advocate again the absolute necessity for such microscopical examinations.

Further records should be kept of the houses where all attacks, especially primary ones, occur, so that evidence which is now wanting may be forthcoming as to what districts and houses are chiefly malarial.

It is, however, recognised by European residents that Jamestown and Christianborg are "unhealthy," whereas Victoriaborg is "healthy;" *i.e.*, the amount of malarial fever in the former is great, in the latter small.

Wherever such a condition of isolation has been secured, it is imperative also that all local breeding places be done away with; for if a case of malaria be imported into this district the fact that sources of anopheles are present renders a certain amount of infection from this case again possible. In Victoriaborg the very few breeding places

occurring among the bungalows, in the rains only, could very readily be abolished. In other places, less favourably situated, a complete absence of anopheles may be more difficult to secure, but those occurring are far less likely to be infected, and their destruction can scarcely ever be the herculean task presented by a large native town with many thousand inhabitants.

If with this condition of isolation at Victoriaborg we contrast the conditions in Freetown, we find that they could hardly be worse. European and native quarters exist side by side, and not infrequently a European may occupy the upper floor of a building of which natives occupy the dark cellars, which are a noxious feature of the dwellings totally unsuitable for Europeans there. The conditions of these houses are quite unsuited to tropical life; they are ill-constructed, surrounded by cess-pits, and have no verandahs. They are, in fact, most ill-adapted for life in a climate which is even more enervating than that of Accra.

In dealing with the question of anopheles in Freetown, we had to consider the conditions as we found them, and, as the most practical means for destroying the numerous breeding grounds of anopheles, we advised drainage. We should, however, lay more stress on the prime necessity for isolation, and, as it is under consideration to erect European dwellings in the adjoining hilly country, we consider that this is the only efficient way of dealing with the extremely dangerous conditions of existence there. We, however, would repeat again that, if this removal be carried into effect, strict attention must be paid to the proximity of native dwellings. It is not the elevated site in itself which will protect the Europeans there, for anopheles, as we have seen, exist in the hill districts of Freetown; it is the removal from the neighbourhood of the infected native. Consequently it is of prime importance that a native village be not allowed to spring up in connection with the European quarter, and the native quarters for servants should be removed as far as possible.

In Freetown isolation cannot fail to be most beneficial, and in the building of new houses, here and elsewhere, sites should be chosen which are as remote as possible from native quarters wherein the danger lies.

S. R. Christophers, M.B.
J. W. W. Stephens, M.D. Cantab.

Accra, Gold Coast,
May 17, 1900.

"Note on Certain Bodies found in the Glands of Two Species of Culex." Received June 8, 1900.

The first (fig. 2) were found in a culex commonly found in the bush in Sierra Leone. This culex was not found in Freetown itself. The bodies occurred in about 10 per cent. of this culex examined.

We have referred to these bodies, which have the characters of sporozoites, in our report on the distribution of anopheles in Sierra Leone. We have not so far succeeded in demonstrating a central stained spot (chromatin) as in sporozoites of *H. præcox*.

The second were found in from 5 per cent. to 10 per cent. of a species resembling *Culex pipiens*, caught in a native village, together with anopheles and other culex species. So far we have found them in only one species of culex. In general, they are few in number, but in some portions of the gland they may be numerous. As many as half a dozen may occur in each globule of secretion. They may also occasionally be seen free in the salt solution. Their chief peculiarity is their straightness. Their nature remains so far undetermined.

J. W. W. Stephens, M.D. Cantab.
S. R. Christophers, M.B.

May 17, 1900.

"The Malaria of Expeditionary Forces and the Means of its Prevention." Received June 8, 1900.

In our report on "The Native as the Prime Agent in the Malarial Infection of Europeans" we have shown—

1. That in all native villages examined by us, from 50 to 90 per cent. of the children were infected with malaria. That a considerable portion of these infected children contained crescentic bodies, which very rapidly took on the spherical and flagellating form requisite for the transmission of human malaria to the mosquito.

2. That in all native villages examined by us anopheles were present. Nor were they absent when breeding places had not been in existence for varying periods up to three months. That, moreover, a certain proportion of these were always infected with the mosquito phase of the parasite. Not only the village as a whole, but each individual hut, in all but rare cases, contained both infected children and infected anopheles.

These conditions have been encountered by us in British Central

[redacted]

Bodies in Glands of Culex.

Fig. 1.—*Anopheles costalis.* Sporozoites of *H. præcox.* Fresh prep.

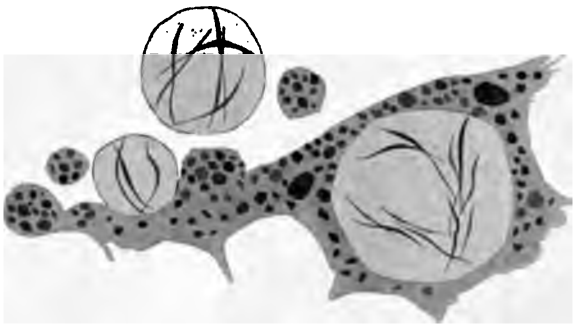


Fig. 2.—*Culex* (?). Sporozoites, nature unknown. Fresh prep.

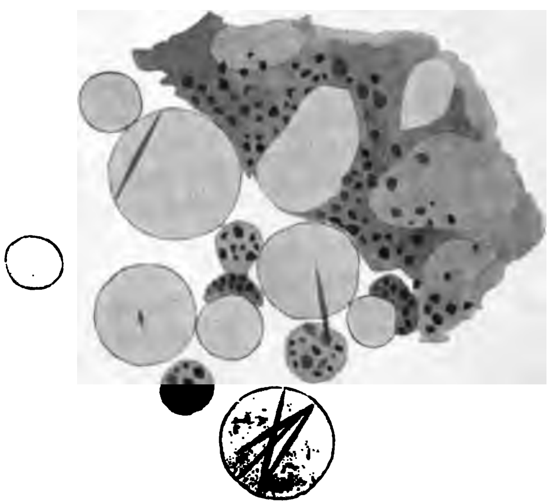


Fig. 3.—Culex pipiens (?). Bodies in secretion. Fresh prep.

Africa, Sierra Leone, and the Gold Coast, and there can be little doubt that they prevail universally throughout tropical Africa. These facts then are sufficient to explain why exploratory and military expeditions are attended with such a large amount of sickness and so terrible a mortality.

Very rarely does a traveller or expeditionary force pass for long distances through uninhabited districts; while even in the most remote, such as for example the Congo forests, villages exist. In a village not only are there food and other conveniences for native porters and even for Europeans, but they are also the only places where it is possible to find a ready-made clearing for a camp. Consequently it is the invariable practice for travellers and expeditions of all kinds to pitch their camps in the village clearing—if not to sleep in the village huts. For the single traveller there is usually no other alternative than to sleep, night after night at the end of each day's journey, in some native hut. Whether a hut is occupied, or whether a camp be pitched close beside the village, matters little; in either case infected anopheles, in greater or less number, are present.

So insidious, as a rule, are the attacks of anopheles, if few in number, that they attract very little attention.

Of the fever contracted in travelling we have in these conditions quite sufficient explanation. When night after night, for weeks or months, men, through sleeping in villages, are exposed to the bites of infected anopheles, it is quite evident that here we have the source of their sickness, and that it is unnecessary to call in the hypothetical influence of uninhabited swamps which they may have traversed. Assertions made by travellers that they have, for many days running, camped by the side of uninhabited swamps, must be received with extreme caution. Such a method of camping is in tropical Africa, to say the least, both unusual and extraordinary.

Infection, then, can most certainly be escaped by a strict avoidance on the part of Europeans of native villages and huts. If native villages are of necessity selected as sleeping places, then, whilst in such places, the most scrupulous care should be observed in the use of mosquito nets, which should be under the vigilant supervision of the medical officer of the expedition. It is essential that they should be absolutely free from holes, and, further, that they should have a protective valance at the level of the body to prevent bites being effected through the net. A further modification of the net in general use is needed to suit the conditions where the traveller uses an ordinary camp bed or, as often happens, sleeps on the ground.

The danger of being bitten and infected in villages is so great that even the use of nets would probably not ensure complete protection. It is essential, therefore, that every endeavour should be made to camp elsewhere than in a native village. Villages are a certain source

of infection, and their avoidance will certainly result in a much diminished amount of malaria in expeditions.

J. W. W. Stephens, M.D. Cantab.
S. R. Christophers, M.B. Vict.

Note.—In a later letter from Mr. Christophers, dated "Accra, 26 May, 1900," further stress is laid on the fact that camping and sleeping in places *remote from human habitations* is in the highest degree exceptional in the case of European travellers and expeditionary forces. This fact, the writer thinks, is perhaps not sufficiently appreciated by persons not conversant with the conditions of African travel.

ERRATA.

In Report published July 6, 1900.

Page 62, line 18, *for* "titræ" *read* tibiæ.

" 72, upper footnote, *for* 'Lancet' *read* 'British Medical Journal.'